THE MATHEMATICAL GAZETTE

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THE MATHEMATICAL ASSOCIATION

AN ASSOCIATION OF TEACHERS AND STUDENTS
OF ELEMENTARY MATHEMATICS

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'I hold every man a debtor to his profession, from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavour themselves by way of amends to be a help and an ornament thereunto.'
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THE TEACHING OF MATHEMATICS IN SCHOOLS: A CRITICISM OF THE ENGLISH EDUCATIONAL SYSTEM*

By L. Rosenhead

In opening this afternoon's discussion I wish to put before you some reasoned criticisms of a number of attitudes and arrangements embedded in the English Educational System; I want to talk generally—not submit a detailed blue-print of some brave new system. During my talk I will ask you a number of questions and I would be grateful if some of you would respond and indicate your views. Although I feel deeply on some of the points I shall raise, I put forward my arguments with diffidence, for I realise that opinions may, and frequently do, differ.

To be quite comprehensive I ought to speak about kindergartens, primary schools, and our three brands of secondary school. Ignorance and modesty, however, combine to turn me from such a task, and I therefore propose to speak on what I think I know best —the young people who, from one point of view, are the "end product" of the secondary grammar branch of the English educational system and who, from another point of view, are the "raw material" of the English university system. From my review of this special group of young people questions will arise, however, which have a direct bearing on the problem of teaching mathematics in English schools generally.

Next, I ought to make it quite clear that I find it impossible to discuss the problem of mathematics in schools without at the same time considering the school reponsibility of laying the foundations of good citizenship. To me the two problems are inter-woven and the second, that of creating good citizens, is by far the more important.

* The opening address in a discussion held at the Mathematical Association Meeting in April 1961.

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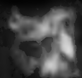

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In this connection I should like you to consider with me the qualities of the young people whom we admit nowadays into our university departments of mathematics. I have heard no criticism of their physical maturity. In fact, students of today seem healthier, taller and, if I may say so, more good-looking, than those who were admitted to universities in my day!

While commenting on physical characteristics it is, I feel, proper to draw your attention to the fact that there has been an increase in the age of admission to universities, which was about 15 at the turn of the century, and which has risen during my lifetime from about 17 to about 18½. There now seems to be a tendency to increase the age of admission still further—to about 19. For potential mathematicians I am sure that this is bad, and should be resisted. Mathematicians possess a special kind of mental acuity which falls off rapidly with age, and I favour the view that, taking present social conditions into account, the optimum age for the admission of young men and women to universities for the purpose of specialising in mathematics, is between 17 and 18. I would value your views on this point. Any agreement on age of admission will affect directly the standard of knowledge and understanding that can reasonably be expected from those who are admitted to our universities, and hence it will also affect matriculation requirements.

Next, despite the expansion of our universities, there has been no considerable increase in the number of really bright mathematical students, but there has been a large increase in the number of those whom I place in the " second category of quality ", and it is about these that I have heard most comment from my mathematical colleagues over the country. The comments are the ones with which I am sure you are all familiar—the students do not understand the mathematical ideas which we, university teachers, consider to be basic to our subject; the students are not skillful in the manipulative processes of even elementary mathematics; cannot grasp new ideas quickly or at all; cannot write simple English clearly and grammatically; and particularly have no sense of purpose—that is, they do not seem to realise that in order to study mathematics intensively they *must* work hard *on their own* trying to sort out ideas, new and old, and trying to solve test problems; etc., etc.

I am sure that you have heard these criticisms before, as well as others to which I have not referred, but I feel that I must ask you to consider these criticisms carefully, and I must also ask you whether the picture arising from them is a fair one—as far as university students of mathematics are concerned? I would value the views of school teachers, who are in charge of these young people just before they cross the mathematical bridge to the universities, and I would also like to hear the views of university

lecturers of mathematics, who take charge of the same young people just after they enter the universities. If in this connection there should prove to be a considerable measure of agreement between the two groups of teachers, one at each end of the bridge, then this agreement would be a serious matter for it would show that, in the opinion of the people most capable of judging, all is not well in this complex of schools, pupils, teachers, examination system, and universities. In fact it might be reasonable for us to start from the assumption that something is really wrong here, for otherwise why these national heart-searchings about syllabuses, general education, specialist education and university selection?

My own views are that it would be quite unrealistic to assume that defects of the kind mentioned do *not* exist. There is so appreciable an element of truth in the criticisms that the early initiation of some kind of remedial counter-measures is urgently required. The *really* critical questions for all of us are—what educational targets should we aim at, and what should we do?

Another question that you will, no doubt, ask yourselves is this. Is the situation which has been described *peculiar* to students of mathematics? What about other students? Well, after having listened to the considered views of many university colleagues over the country, I tend to the view that the picture has a fair measure of truth in relation to a *very appreciable* number of university undergraduates, no matter what they study.

A couple of years ago I was present while there was an exchange of views between some school teachers, and one university professor of mathematics apart from myself. We talked about the question I have broached today, and there seemed to be agreement that there was some truth in the criticisms. One of the school teachers then turned to my colleague and asked, " Well, how should we remove these defects? " and the answer was " I haven't the faintest idea—surely that is a problem for you as school-masters ". At that time I thought that the answer was an evasion of the issue, but on reflection I came to the conclusion that there was much wisdom in the reply. I believe that school teachers have very important work to do—to produce in young people that outlook and frame of mind which we are all looking for, both in and out of school, and it is the duty of university teachers to build on that outlook and attitude, assuming it to exist, and create *specialists* within the atmosphere of university study. The answer of the colleague I mentioned, if carried to its conclusion, points to the two mutually-complementary spheres of responsibility of schools and universities, and to educational targets for both of them.

The present critical attitude of university lecturers in mathematics may be due to the fact that *they* are not inspiring teachers—

that may be so. But I am equally sure that this is not the most important part of the situation. I can assure you that, from my own knowledge, university lecturers are now being compelled to devote themselves to teaching, even of an elementary kind, much more seriously than in the recent past. The fact that they feel compelled to do so speaks for itself.

Some university departments of mathematics are now refusing categorically to accept young people without high personal and academic qualifications; these departments say that they are "maintaining standards". Other university departments accept students for honours courses in mathematics extending over the 3 years which are traditional in this country. At the same time these departments permit students to repeat those years of study in which they fail. Sometimes the departments run a "preliminary" year outside the 3-year honours course. This means that these departments quietly run 4-year honours courses side-by-side with the publicised 3-year ones. Still other universities openly arrange 4-year honours courses for these applicants of the second rank. In fact a two-stream system is developing in the country, a 4-year stream and a 3-year stream, both leading to the goal "Honours in Mathematics".

The mounting pressure to enter universities undoubtedly means that there has been, and will continue to be, a disproportionately large increase in the number of these second-rank applicants. I am sure that there would be a justifiable public outcry if universities were to refuse to admit as many as they can of the young people in this category. Taking all these factors into account I see no escape from the necessity of having to provide in this country 4-year honours mathematics courses for an increasing number of young people. I do not shake my head at this. I accept it as a challenge. I do more—I welcome it—but from another point of view. In fact, many of those whom we *now* admit for a 3-year honours course would benefit considerably, both personally and academically, if they had to spend 4 years in university surroundings instead of 3. I find, for example, that an increasing number of young people show signs of mental exhaustion shortly after they are admitted to our universities, as well as obvious signs of a lack of what I call a "sense of purpose"—no real understanding of the significance of "studying at a university".

While science is delving deeper and deeper into the mysteries of our world, and while through technology we multiply the fields in which we increase our material resources, the principal pressure to provide specialist training falls on the *universities*, not on the schools. A very appreciable part of that pressure falls on those departments of mathematics which have a hand in the training of scientists and

engineers. It therefore seems to me quite strange that while it is agreed national policy to increase the length of *school life* there has been no recognition of the need to increase the time spent on subsequent specialist university studies. In this connection some decision is required on the correct balance between time spent in school and that spent on subsequent specialist studies. On this point too I would welcome the views of those who are here today.

I take it for granted that we are all agreed that it is essential for our country to create for itself a population more critically-minded and generally more knowledgeable than in the past. This need arises from the demands of our times, in which the social structure of our country is changing rapidly, and in which there is a growing need for its members to exercise greater wisdom and mental maturity than was necessary in the past. In addition there is a greater demand for material resources. The question then arises—*Through which agency or agencies has this wisdom to be imparted*? Is not the answer self-evident? Which agency has charge of our youth during the most formative years of their lives? Surely—our schools! And it is upon these institutions that there rests the main responsibility for laying the philosophical foundations of good citizenship, to supplement the practical lessons in citizenship learned in the home.

In principle, primary schools and secondary schools *do* devote themselves to this task, but as far as my own experience goes, which is almost entirely associated with secondary grammar schools, I can see little evidence of improved qualities of citizenship among the young people who are admitted to our universities. In fact, many of our students seem to be mentally less self-reliant than were their counterparts 20 or 30 years ago.

The student defects which my mathematical friends have mentioned from time to time relate mainly to questions of outlook and sense of social responsibility. The view has been expressed that if only students would enter their universities with a greater sense of purpose they would make greater progress in their mathematical studies. But what do we see? Instead of devoting themselves to this matter secondary grammar schools devote themselves to specialist studies—specialist mathematics, specialist physics, and so on, in their sixth forms. A careful investigation recently made by Mr. Peterson of Oxford seems to indicate that, if we consider school-time and private-time together, young people devote up to 90% of each working week to professional, specialist studies—and how can such a system enable our young people to equip themselves for broader mental horizons? It is *physically* impossible for them to do so. What we have in schools is specialisation with a vengeance —and, as far as my experience goes, it does not even result in increased mathematical scholarship.

We find, for example, that students have been injected with mathematical ideas which they have not fully grasped. We find that students have been drilled in problem-solving rather than in understanding, and not very efficiently at that. We find that many students are mentally tired when they come to us, and that they relax their efforts when they enter universities, rather than advance with increased mental and physical vigour.

I have only little experience of what happens in mathematics in secondary modern schools—but I know something of what happens in secondary technical and secondary grammar schools. Here I feel that much depends on the attitude of mind of the teacher—and if only teachers could be given a more reasonable task to perform I am sure that there would be an immediate release of their interest, energy and excitement—to the ultimate benefit of all concerned.

Instead of spending four years in the lower school, as was the general rule until relatively recently, five years is the present order of the day. In the upper school the two years which, only a short time ago, were considered sufficient for advanced and scholarship papers, has now " automatically " been increased to three for those who wish to present scholarship papers. There is also a trend to increase the period of sixth form study still further—to four years. And what is happening is that in the third and fourth years in the sixth, young people just keep on repeating the work that they should have completed in two years.

There is another aspect to this question—the social one. There is no doubt that young people of today, of 18 years of age, are physically more mature than were their counterparts some 20 or 30 years ago. Appreciable numbers of our young people leave school, however, at about 15, and within a matter of days find themselves earning appreciable sums of money each week. The sense of power and excitement that this engenders induces the more energetic and uninhibited of these young people to indulge in exploits of the kind that have given rise to the phrase " juvenile delinquent ". I believe that these young people enjoy more than anything the sense of power and excitement which their money makes available to them. Do you imagine that their more studious colleagues, in school, do not sense the excitement that is in the adolescent air? They too stir, uneasily sometimes, and would like to taste excitement and new experience—instead of which, even as they grow taller and broader, they are made to retain the mantle of school-children in an atmosphere of constant intellectual direction. I feel that it is wrong to keep such children too long in the atmosphere of school; two years in the sixth should be ample if the work there were directed towards (i) the broadening of mental horizons, (ii) the training of character and (iii) the introduction of only the elements of specialisa-

tion. After that, almost every young man and woman ought to move out of the atmosphere of school into surroundings that are new and full of challenge. In this way it might be possible, at an age between 17 and 18, to satisfy the natural desire for change and excitement, that we see everywhere among our young people.

My complaint against the present curriculum of secondary grammar schools is that it devotes itself to what I call *excessive specialisation* and does not concern itself sufficiently with the more significant problems of society. It is, of course, highly educational to have young people introduced to the idea of specialisation—study in depth—but the school curriculum, in my opinion, goes well beyond the limits of common sense in this matter. The teaching of mathematics in schools, for example, is nowadays divided into several streams; mathematics for potential mathematicians, mathematics for potential science or engineering specialists, mathematics for potential statisticians, mathematics for a mixed group of young people, mostly girls, who have not been given an opportunity of learning physics or some other experimental science. Could specialisation be more foolish, especially when relatively little guidance is given on the more important problems of what constitutes good citizenship, and the significance of many of the things that are happening in the world today?

Minor pruning of G.C.E. syllabuses, and new schemes for grading results, have been acclaimed, but these are matters of only secondary importance. The real problems, the significant ones, those relating to general outlook and social responsibility, remain virtually unchallenged. If these problems were attacked successfully results of value would be seen not only in general education, but also in specialist studies.

Further, this *excessive* school specialisation in mathematics is under the care of teachers of mathematics who, however well-meaning, are out of touch with modern developments in many of the fields of pure mathematics, applied mathematics, theoretical physics, theoretical chemistry and analytical engineering. Is it then surprising that the young people who come to us from secondary grammar schools seem to be imbued with ideas 20 or 30 years out of date? The surprising thing is that the students are not even efficiently drilled in problem-solving.

What is required is that the schools should produce young people of quality, that schools should not indulge in excessive specialisation, and that at the appropriate psychological age pupils should leave the atmosphere of schools for the freer and more challenging surroundings of institutions of higher learning.

The Crowther Report extols the principle of specialisation of

studies in schools as a traditional aspect of the English education system. Now we all value tradition, but I feel strongly that we should not allow it to take such a firm grip on our minds, that it prevents us from dealing with the special problems of our present day world. Tradition should play its part in society but should not dominate it.

One always likes to blame something or somebody for a state of affairs of which we disapprove. "Teachers" immediately spring to mind in this connection, and the corporate body of teachers forms a nice, sitting target which cannot retaliate. My own views are that the teachers are not *primarily* responsible for the present situation. In this connection I generalise my own experiences to say that teachers, especially those of mathematics, are competent, sensible, sober, serious-minded people, who frequently possess a sense of humour—but the trouble is that they have been given a task which is out of tune with the times in which we live. It is the *task* placed on their shoulders which defeats them—that of actually producing specialists in schools! They should not be required to do so. What in fact they do produce are pseudo-specialists, and not very well educated ones at that. What is required is not censure of our teachers, but censure of the system that forces our children into this mould.

University selection systems come in for a great deal of criticism—largely from those who are unfamiliar with them. University selection boards are blamed for the fact that, apparently, so many of those whom *they* admit, suffer academic set-back of one kind or another. What people seem to forget is that there is no fool-proof method of selection—and that selection is based, essentially, on human judgment. As far as this judgment is concerned it should perhaps be noted that only about 1 in 7 of those admitted to universities fails to achieve some kind of academic goal; put the other way it means that 6 out of every 7 do, in fact, leave the universities with some kind of academic qualification. But let us turn to the secondary grammar schools. Here a much-debated system of selection has been in operation for some time—the 11 + examination. I wonder if it is realised that, after having separated the so-called bright children of this country into the grammar school stream, a *very large number* of these bright children fail to satisfy elementary tests even in their mother tongue, the *English language*, and also in mathematics, etc., etc., etc., 4 or 5 years after they are separated from the so-called not-so-bright children. In fact, approximately 2 in every 5 of those who at the age of 11 are considered to be our brightest children are relegated to the intellectual dust-heap about 4 or 5 years after they are admitted to grammar schools. What kind of an educational system have we in this country, that

apparently ignores such grave defects, and concentrates instead on specialist work more appropriate to advanced training?

In conclusion I should like to summarise my attitude in a somewhat different way. It is, I believe, not unusual to sense that about the age of 15, many young people start to sit up and take special note of the outside world. They start to regard their elders, themselves, members of the opposite sex, clothes, etc. in a new kind of way. This stage marks for them the opening of new horizons. Now we, instead of allowing this interest to develop naturally, and instead of trying to guide it, consciously restrict their vision. We more or less compel most of our brightest young people to decide, at the age of about 14 or 15, the intellectual pattern of their entire future—and we start them on the road of vocational study. By our examination arrangements we tell them in clear terms that specialist study is very important, general education not at all. At about this age most young children can easily be directed, but changes come quickly during the years of adolescence—that period of uneasy development full of doubts and uncertainties. To many of these young people the universities appear over the horizon as places of refuge where they will be mentally more free. They expect that in the universities they will find the broadening of their vision, but many of them are disappointed. When they reach the universities, instead of being able to let their fancies roam, they find that they are compelled to specialise even more fiercely than before. This state of affairs is a consequence of the implicit acceptance of three basic assumptions—that in schools the beginning of specialist training is more important than general education, that formal instruction in universities should be concerned *only* with specialist training, and that an honours course, for artists, scientists and engineers, must not be longer than three years.

All this goes against the grain of the mental development of young people, as I understand them, and I therefore believe that the system we have today cannot be *expected* to produce results of value *except* in the very ablest of our students. We have a system designed for the ablest, but we apply it not only to them, but also to an increasing number of those in the second rank of quality. This is illogical, and logic always triumphs—in the long run. Unfortunately time is not on our side. What we need is a clear re-assessment of the functions of our *present-day* schools and our *present-day* universities, not those of previous generations; we also need clear statements of our objectives, and we must *all* work with a will to put our respective houses in order, and even alter our existing system of education if that should be found to be necessary.

L.R.

The University, Liverpool

MODERN MATHEMATICS AND THE SCHOOL CURRICULUM*

By M. H. A. Newman

At the International Congress of Mathematics to be held in Stockholm in 1962 there is to be a discussion, in the Section on Mathematical Instruction, on modern mathematics in the school curriculum. This has been the occasion for a preliminary airing of the subject in many of the countries concerned, and an important effect of the Stockholm discussion will no doubt be to direct attention beforehand to this important problem.

I shall try not to speak this morning either as an advocate or as an opponent of the introduction of such subjects, though I fear that some of my pet hobbies and pet aversions will come out. The theme is close to that of the address which I had the privilege of giving to the Association two years ago, and I hope I may be pardoned if there is some overlapping between the two.

" Modern mathematics " is a pretty broad term, including many domains in which old methods, used in a new way, have led to fresh discoveries; but what most people's thoughts now naturally turn to when they hear this phrase is the axiomatic or " abstract " mathematics typified by *algebraic theories*, about groups and fields, Boolean algebra and so forth.

The first algebraic structure not consisting of numbers that is likely to be met by pupils is a vector-space. It is an excellent example for them to start from. They need little more than the notion of the resultant of two vectors, and the multiplication of a vector by a scalar. They are not likely to raise any objection to the use of the symbol $P+Q$ for the resultant, or λP for the vector with the same, or opposite, direction as P, and having $|\lambda|$ times its length.

But, we may ask, why not call the resultant $P \, . \, Q$ or P/Q or P^Q? The answer is that this "+" behaves as "+" should: if we do algebra in the familiar way with such expressions as

$$3(P+Q)+5(P-Q),$$

taking out and putting in brackets and so on, the answer will be that which the geometrical meaning requires. It is then natural to ask what *are* the properties that justify our giving the names $P+Q$ and λP to these combinations; and with that, we are launched on an inquiry into the axioms of vector-spaces.

* A lecture delivered at the Annual Meeting of the Mathematical Association in April 1961.

Let us follow this example a little further. If a class can be led to agree that all the things we normally do with vectors depend on just the two facts that they can be added together and multiplied by a scalar, according to the usual laws, they may be willing to grant that the name *vector-space* should be given to any set of things that can be added, and multiplied by a number, so that the result is a member of the set. They will then find that they are committed to granting the name to such unexpected sets of things as all the power series in x, and all solutions of $y'' + 3y' + 2y = 0$. In this way they may be launched unawares upon axiomatic mathematics, as M. Jourdain was launched upon talking prose. The rather unhappy effect of the label " abstract ", which is so firmly attached to these theories, may be counteracted by seeing them exemplified in so simple an example. All that the word " abstract " means, indeed, is that these theories have not just one interpretation, but a whole lot of different ones. The practical advantages of a unified treatment of various sets of mathematical objects with a similar structure, are also well brought out: the geometrical picture of a vector suggests various arguments and notions (for example of subspaces of lower dimensions) which turn out to be valid for the other new kinds of vector space.

Another example which is very attractive if it can be successfully explained is the theory of the finite fields, J_p, formed by doing ordinary arithmetic but keeping only the remainders relative to some fixed prime number p. Within the field J_7, for example, whose only members are 0, 1, 2, 3, 4, 5, 6 we have $4+5=2$, $4+3=0$, $2.4=1$ and $6.6=1$. It is a surprising fact to pupils that, provided that the use of inequalities, which do not exist in these theories, is avoided, all the processes and rules of rational algebra lead to correct answers, including the solubility of the equation $ax=b$ if $a \neq 0$, although only the p members of J_p are available as solutions. A proof that this is so can only be given if the question has been made definite by deciding what the ordinary " processes and rules of algebra " are; and this again leads to axiomatic inquiries.

This theory demands perhaps a little more from the pupil than the vector-theory, but it has been tried out successfully by at least one teacher, who I hope may say something about it in the discussion. This example also has the advantage of providing simple and (once the ideas are thoroughly understood) very direct proofs of many theorems of the ordinary, elementary, theory of numbers. It is, of course, also the simplest possible example, after the real numbers themselves, of a fundamental concept of modern algebra (a *field*).

Another entirely different algebra, favoured by some as a point of entry, is the so-called algebra of sets, or Boolean algebra. Here there are two operations, somewhat analogous to + and . , which

were indeed so called by Boole and everyone after him, till about 25 years ago. But they behave sufficiently differently to have now been given different names. If A and B are any two sets of things, $A \cup B$ denotes the *union*, that is the set of all the things that are in at least one of them; and $A \cap B$ the *intersection*, or common part. These operations are commutative, associative and distributive, just like + and . , and there is the empty set 0, satisfying $A \cup 0 = A$, $A \cap 0 = 0$. There are also some marked differences, of which the most important is that there is no really serviceable analogue of subtraction, i.e. no solution of $A \cup X = 0$ for general A. However, the rules are soon learnt and there are plenty of agreeable manipulative examples, of which, moreover, it is quite easy to see the meaning by drawing simple diagrams.

My only reservation about this theory would be that not much seems to come out of it at this level that cannot be done just as well without it. It may be a good first example of a new kind of algebra, but too much time spent on it may leave pupils with a feeling of flatness. If axiomatic mathematics is to be done in school it is important that some at least of the subjects chosen for treatment should lead to substantial calculations with worth-while results. I must point out that I am here approaching questions on which there is a certain cleavage of opinion among those teachers who wish to introduce some of these new concepts. To some it seems that all that should be aimed at is to introduce the ideas and make them intelligible by carefully chosen examples which have no purpose but this. The pupils, having been led by a careful and gradual analysis to formulate the axioms of, say, a field, will then establish these thoroughly in their memory and understanding by checking in detail that some of the structures which are candidates for the title of " field " do, and others do not, satisfy the rules; and this is more or less the final aim and purpose of the course. It is natural when this is the aim to look for as simple a set of rules as possible to use as a first example, and for this the claims of the *theory of groups* are strong. In this theory there is only one operation, usually called multiplication. If we consider groups of *operations* such as movements of a rigid body about a fixed point, and if multiplication means " and then ", the associative law is automatically satisfied: " do A and then B and then C " needs no brackets. All that is needed, to establish a set of operations as a group, is to verify that the combination of two operations is itself an operation of the set, and that each element A has an inverse, A^{-1}, in the set which, performed after A, restores the *status quo*.

Such ideas, and the verification that, for example, the six operations

$$I \text{ (identity)}, \quad \frac{1}{x}, \quad 1-x, \quad \frac{x}{x-1}, \quad \frac{x-1}{x}, \quad \frac{1}{1-x},$$

on a number x form a group, should be within the grasp of a sixth form set; but it may be found rather difficult to move on from definitions into theorems and non-trivial *uses* of group-theory. The very sparsity of the structure makes worth-while applications hard to find. For it is, of course, a mistake to suppose that in mathematics, logically simple = easy. On the contrary, the richer the structure the easier it is to move around. For this reason I feel somewhat doubtful about group-theory, a tough subject, as a suitable school topic.

Axiomatic theories are *methods* for solving large classes of problems by the same piece of apparatus. Unless they can be seen in action they have not really been understood as mathematics, although the logical analysis may have been followed to the end. I sometimes have the feeling, as I browse about in the literature, that some teachers regard it as a scandal, to be hushed up if possible, that calculation is the heart of mathematics. The best that is said for it is that " of course the technical side must not be too much neglected ". This was not the view of Archimedes or Descartes, each of whom called one of his greatest works by the title " The Method ". Are we not perhaps overdoing explanation a little? I remember with sympathy the student who said to me " I seem to get on with limits all right, it's the explanations I find difficult ". We should, then, prefer axiomatic subjects in which explanation and understanding do not drown out the uses. In the presentation of axiomatic theories applications of real substance are also a safeguard against a danger of which I am sure everyone here is well aware: that if concepts of great generality are introduced before pupils have progressed far enough to have in their heads an adequate stock of more elementary mathematics to serve as examples, they may attach no real meaning to these notions. A really varied set of applications, not too easy in themselves, is the best possible way of bringing to light the misunderstandings which may lurk behind considerable facility in doing formal exercises which lie too near the definitions.

Some teachers fear that many pupils will never be capable of taking in these abstract ideas at all, that they are too difficult for all but the few. It is hard for us to set aside the prejudices created in all of us by the fact that until recently these were considered " advanced " subjects even in the universities. But if the experience of the universities is any guide, it shews that, provided that students are not frightened off by over-heavy preparation and explanation, they take perfectly easily to these theories and find them easier than classical analysis. A course on the theory of groups is now taken in the first year in at least some universities. However, the proper way to answer this question is by experiment.

I have given most of my time to discussing the problem of introducing axiomatic theories, because this is the most radical and the most difficult of the possible innovations; but there are a number of subjects, which are modern, but not abstract in the sense we have been discussing, which might well be tried out. I will only mention one: the geometrical topology of surfaces, treated in a thoroughly concrete scissors-and-paste way. The method of cutting down a surface into its fundamental polygon, and so calculating its genus, should be within the reach of a school treatment. The geometrical side of school mathematics is sadly depleted at present. Projective geometry is being squeezed out of the university courses and a generation of teachers will soon come along that knows not homographic pencils. What is left in the school course is $y^2=4ax$, etc., etc., the dustiest corner of school mathematics, which does nothing to develop geometrical imagination. Perhaps some geometrical topology is the answer.

There is one question which must occur to everyone, about which I have said nothing: where is the time to come from? This is a separate and very large question. If we try to discuss, say, the rival claims of projective geometry and ϵ-analysis to receive the first blow of the axe, it will take us too far from the merits and difficulties of modern mathematics itself as a school subject, which is our assigned theme this morning.

M.H.A.N.

MATHEMATICS BEGINS WITH INEQUALITY

By R. C. H. Tanner

"First books" on mathematics usually begin with *numbers*. Some histories of mathematics say in so many words: Mathematics *begins with numeration*. I believe this is true, literally, if mathematics is considered as a set of signs, with rules and relations between them. That is to say: the *history of mathematical notation does begin with the numerals*. The earliest known mathematical signs are no doubt number-signs, and history of mathematical notation has in fact little else to discuss but numerals up to the 14th century of our era. Of present-day mathematical signs, + and − appeared first in the 15th century, = in the 16th, $<$ and $>$ in the 17th.

It is clear that this gives *no adequate framework* for the *history of mathematical ideas*. A great deal of basic mathematics had been worked out before the 14th century A.D.! It was expressed in *words*, abbreviated, but not symbolised, except as regards the

numerals themselves. The *history of mathematical ideas* cannot then begin with the first mathematical signs, unless these also represent the first notions in mathematics.

Now this is obviously *not* the case. The *most primitive* number-system implies an acute sense of what is meant by *equal* and *unequal*, by *more* and *less*. If we do not want to argue about aboriginals, we need only go to the nursery, to remind ourselves that *more* is almost the very first intelligent expression a baby learns: *more*, meaning *one more*, another one, and *no more*, meaning *enough*. Exactly the ideas on which the primitive process of counting is founded.

Besides, there is of course the evidence of language; as far as I am aware even the most rudimentary language contains comparatives, that is, the ideas of more and less are not only older than numerals, they are *older than any language*. Of course, animals have notions of greater and smaller too, and very few animals are known to have a developed sense of number. But perhaps you will agree with me, however common, ideas of *more and less are mathematical ideas*. They are more primitive than the idea of *equal*, and equality is in practice much more difficult to assess than inequality. In very many cases equality cannot and need not be finally established, we only know that *no surplus* and *no defect* has up to now been detected. In other words, *equal* means *neither more nor less*; it is a *negative* and *derivative* notion.

On this account I should be tempted to alter the current opening phrase and say " Mathematics begins with inequality ", and then see what this declaration commits me to.

It commits me to re-examine the place of inequality in the teaching of the human child, in the popular presentation of mathematics, as well as in that framework of history of mathematics, the history of mathematical notation, and to relate the three assessments with the place of inequality in mathematics at its present stage of development.

Modern mathematics is unthinkable without inequality and the inequality signs. Very few very minor topics can do without, or only by masking the true nature of their material.*

In practice, a statement of equality is an approximation, that is, an inequality, single or double. Limit relations are inequalities, an infinity of them. Validity conditions are usually inequalities. Even an absolute equality is as often as not proved by *a reductio ad absurdum*, that is, by discussing the related *inequality*. More than this, the true interest almost always lies in *this discussion*, not in the end result in equality form, which tells only part, a small part, of the story. *The equality indicates a boundary*, but we are really con-

* Linear equations can, infinite series pretend to, set theory substitutes inclusion to inequality. Order and inequality coincide. " Mathematics begins with order ".

cerned with what lies *inside* and *outside*. The equality is like a fine ceremonial dress, beautiful for show; but you get into your shirt-sleeves for the real work. In fact that seems to be the keynote of the situation; we like to present our *finished mathematics*, mathematics for show to the public, as much as possible in *equality form*, but in the mathematical workshop inequalities are the standard tools. This discrepancy may be responsible for the once current heresy that mathematics is just one grand tautology: "finished mathematics" has been taken as representative of the whole body of mathematics and as characterising it all in essence. Perhaps some glimpse behind the scenes would have brought mathematics much nearer to the general public, and even more decisively, to the school-child. Instead of which, inequalities have been erased from *many algebra courses that used to contain them*. Is it perhaps that inequalities belong to the "rough work" that so many like to hide? A trade secret not to be too widely divulged? Or is it thought that inequality theory is too like Euclid, really a branch of logic, outside mathematics proper?

If not, then there is a case for emphasising the mathematical content in the exercises and games in the nursery which ask what is *bigger or smaller*, without the much more advanced and tricky question of what is *the same*, or *how much* it is. There is an inexhaustible fund of useful material here. Surely the mathematics teacher in later years would find his or her hand strengthened, if more of *what the child already knows* can be pointed out as belonging to the realm which is now being explored? It would be very interesting to know what Piaget's pioneering experiments show in this respect.

In the popular presentation of mathematics, the question whether inequality theory is *fundamental* to mathematics seems to fade in comparison with its indispensability as a modern workshop tool. There is surely no understanding mathematicians unless you see something of them in undress.

Finally the evidence of the history of notation seems to be of extraordinary significance. The data are of course still far from final; it is a difficult and neglected subject, and there is no complete agreement between different contributors. It is however, to say the least, highly suggestive that the deepest-lying of the basic notions was the last to be symbolised, whereas numbers must have had signs almost as soon as thought of. We also find that the invention of $+$, $-$ and $=$ were dictated by pure expediency, but $<$ and $>$ were essential aids in thinking for one man who found clear thinking often terribly hard.

Imperial College R.C.H.T.

THE EDUCATION OF MATHEMATICIANS

An alternative to the University degree course in Mathematics

By J. Topping and J. Crank

A significant modern trend is the increased use which industry makes of mathematicians. Only twenty years or so ago almost all university graduates in mathematics had to find careers in teaching, in schools, technical colleges or universities; a few became actuaries or entered the Civil Service. To-day the situation is appreciably changed; many more of the new graduates in mathematics enter industry or research establishments. (This is one reason why there is a shortage of mathematics teachers in schools). In 1959, just over three thousand graduate mathematicians were employed in industry, public corporations and in central and local government. Nearly eleven thousand others were employed in some kind of teaching. Although more than three quarters of all mathematics graduates are still to be found in education, the number of those who are not has increased by nearly two-fifths in the last three years. This increase is likely to persist in the future as the complexity of industrial research and development grows. It is significant that recently there have again been moves to establish an Institute of Mathematics, on the lines of the professional organisations for scientists and engineers.

Alongside these developments, and closely related to them, have occurred changes and advances in mathematics, which have so transformed the subject and its applications that some treatment of them should be incorporated into new educational schemes. We are referring in particular to the advances in numerical methods, associated with the name of Hartree and others, and to the increasing use of analogue and high-speed digital computing equipment. There are also the developments in statistics, and the widening use of statistical techniques in science and industry.

Most mathematicians in industry may be roughly grouped into three categories; classical applied mathematicians and theoretical physicists, numerical analysts and programmers, and statisticians. Applied mathematicians and theoretical physicists are applying the principles of mechanics, aerodynamics, elasticity, electromagnetism to design problems associated with aircraft, nuclear reactors, radar navigation systems and so on. Their job is to express these problems in the form of mathematical equations which often the numerical analysts and programmers have to solve with the aid of some electronic computing equipment. Statisticians are largely employed on quality control work, market research or as members of operational research groups. The employment of mathematicians in the

commercial world is also being stimulated by the mechanisation of stock control, wages and salaries, and automation in general.

Some university courses in mathematics have been modified to take account of the changes adumbrated above, and when the Diploma in Technology was established some Colleges of Technology took the opportunity of designing new courses in mathematics, as well as in other subjects, on rather different lines from those traditional in universities.

The Diploma in Technology

It should be explained that towards the end of 1955 the National Council for Technological Awards was established as an independent self-governing body responsible for creating and administering technological awards of high standing. The first memorandum published by the Council in May, 1956, indicated that a Diploma in Technology would be awarded to students in certain Technical Colleges who successfully completed courses approved by the Council. It was always intended that the courses recognised for the Diploma in Technology should " be equivalent in standard to honours degree courses of a British University." Some professional institutions have accepted the Diploma in Technology as a qualification for corporate membership on the same basis as a university degree.

The courses in Brunel College are sandwich courses. The students spend six months in the College each year followed by six months in industry, and the whole course extends over four years. The alternation of academic study and practical experience in industry, provided these are properly correlated, is an arrangement which for some students is likely to be fruitful and conducive to the fuller development of their powers.

Each Diploma in Technology course is broad-based; not only is the study of the particular technology deep enough to provide a discipline of the quality usually associated with university degree courses, but the course includes the sciences allied to the technology, an introduction to industrial organisation, and some general studies. In addition, each student undertakes in his final year a project which generally is closely linked with his experience in industry and provides him, it is hoped, with the discipline and excitement of working on a problem which for him has a research element in it. It is intended that the teaching should make use as much as possible of the student's growing industrial experience; it should specially suit those who find the university lecture system too severe and impersonal. Besides formal lectures there are discussions, tutorials and seminars. Throughout the course each student has the personal guidance of a member of the staff who acts as his tutor not only in the College but whilst the student is in the firm, and keeps closely in touch with him and his industrial supervisors.

A Diploma in Technology Course in Mathematics

The course is designed for those who wish to become mathematicians in industry, but it should be emphasized that it is not narrowly vocational. It includes mathematics, physics, electronics and some general studies.

No attempt is made to cover in detail *all* the mathematics which a mathematician in industry might find useful; that would indeed be impossible. One main aim is to help the student to gain such experience and understanding of mathematics, and its relationship to science and engineering, in certain carefully chosen fields, as will enable him quickly to learn and to be able to apply new techniques. Some parts of pure mathematics, such as projective geometry and theory of numbers, are omitted. Emphasis is placed throughout on numerical methods supported by practical work with calculating machines.

In the first three years of the course there is a good broad foundation of pure mathematics and classical applied mathematics. This is supported by courses in numerical methods and statistics; the former include the systematic solution of large numbers of simultaneous equations, and of differential equations both ordinary and partial, in addition to the basic techniques of error analysis, finite differences, interpolation and curve fitting. Students are introduced to computing equipment, both digital and analogue. The statistics deals with correlation, tests of significance, sampling, design of experiments and an introduction to queueing theory. Physics is included in each of the first three years of the course, with electronics in the second and third years. There is no intention to train experimental physicists, but it is regarded of importance that the students should be familiar with the methods and instruments of physics, besides being good theorists. The inclusion of electronics needs no explanation. One special emphasis in the teaching is the formulation in mathematical terms of physical and engineering situations. The general studies are divided formally into English, Social Studies and Fundamentals of science, but they are best regarded as a whole and are an important element in the educational scheme.

In the fourth year, there is some specialisation in one of three main sections Mathematics, Computation, Statistics, which might be summarised as follows:

Mathematics: special functions and polynomials, transform methods including stability and performance of linear systems, tensors and applications, integral equations, generalised dynamics and calculus of variations.

Computation: linear algebra (latent roots and vectors of matrices), ordinary and partial differential equations (accuracy, stability, methods for high-speed computers), orthogonal polynomials (es-

pecially Chebyshev), analogue computing or programming for digital computers.

Statistics: quality control, time-series analysis, queue theory, orthogonal polynomials, programming for digital computers.

Students, besides choosing one of these as their main field of work, carry further and deeper their study of one special subject chosen from elasticity, fluid flow, electromagnetic theory including aerial design and waveguide theory, nuclear reactor theory, mathematical programming. In addition, they undertake a project which introduces them to original work and the new activities associated with it.

Industrial Training

Some of the ways in which this mathematics course is different from a university degree course will now be clear; one other most important difference is the inclusion of alternate periods of industrial training.

This training is as closely related to the student's studies as possible, and each student is expected to tackle the sort of problems industrial mathematicians have to face, working in company with or under the direct supervision of other mathematicians. At least a part of one six monthly period may include experimental work. So far the College has had only one group of mathematics students working in industry, but even this limited experience fulfilled in many ways our hopes of how such training might influence for good the development of the students.

Which Course?

The entrance requirements for the Diploma in Technology course are passes in mathematics and physics at the advanced level in the General Certificate of Education examinations and in three other subjects at ordinary level, and so are roughly equivalent to those for university honours courses in mathematics. High ability in mathematics is essential for both types of course.

Those boys and girls from Grammar Schools and Public Schools, who wish to specialise in mathematics and have decided to follow an industrial career, will find the Diploma in Technology course interesting and attractive, especially if they have a feeling for numerical mathematics and an interest in electronics and computing devices. They will get an early introduction to industrial mathematics, and the quickening experience of meeting practical problems and their methods of solution.

Those who wish to become teachers may go either way; if they wish to teach in Technical Colleges the Diploma course will be specially suitable.

Financially the Diploma in Technology course is attractive.

Industry-based students are paid wages by their firm throughout the time they are in the College and with the firm; College-based students are paid wages by the firm during the periods they are in industry, and whilst they are in the College they are eligible for grants on the same basis as University students and may hold State Scholarships or other awards.

Brunel College J.T. & J.C.

POLYGONAL KNOTS

By James K. Brunton

The simplest overhand knot, if tied in a strip of paper and carefully pressed flat, forms a regular pentagon (Fig. 1). All higher order polygons can be produced in this way with the single exception of the regular hexagon which requires two strips. The reason for this exception is interesting.

Fig. 1

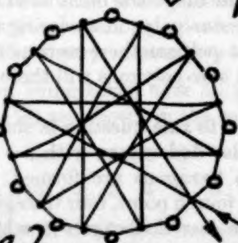

Fig. 2

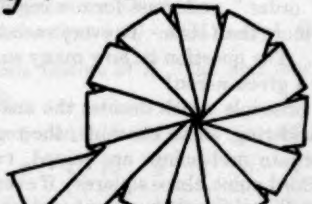

Fig. 3

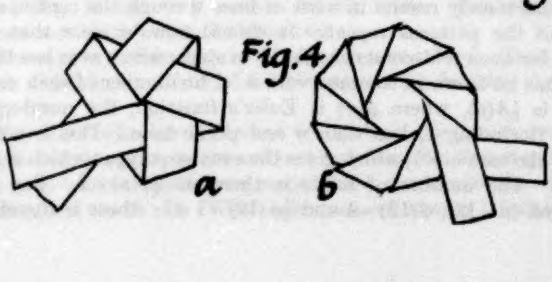

Fig. 4

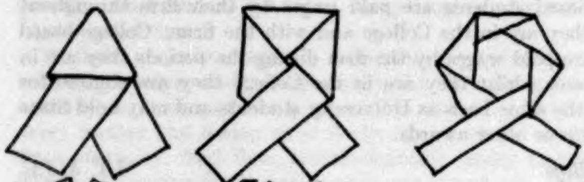

Fig. 5

Fig. 6

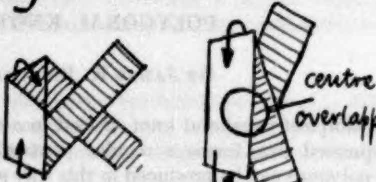

Fig. 7

Consider a knot in the form of a regular polygon formed from a single strip. The mid-line of the strip will lie along a continuous set of diagonals of the polygon whose vertices are the mid-points of the edges of the original polygon. (The actual sides of this polygon will also form the mid-line of a strip, but such a strip will obviously not be knotted.) Where the strip is creased to form an edge of the original polygon, the mid-lines of the two parts must be equally inclined to the crease; hence all the diagonals must be of the same " order " and must form a regular star-polygon enclosing the centre more than once. To every such star-polygon there corresponds a knot. The question is, how many such star-polygons will there be within a given n-gon?

The principle which decides the answer to this question is shown by considering, as an example, the regular dodecagon, with $n = 12$. If alternate mid-points are joined, two hexagons are formed; if every third point, three squares; if every fourth point, four triangles. But joining every fifth point produces a star dodecagon in which every mid-point is visited in turn. (Fig. 2). Finally, joining every sixth point merely results in a set of lines through the centre, and after this the patterns repeat. It should now be clear that the interval between mid-points should be m steps, where m is less than $\frac{1}{2}n$ and has no factor in common with n. The number of such numbers m is $\frac{1}{2}\phi(n)$, where $\phi(n)$ is Euler's function, the number of integers (including 1) less than n and prime to n. This however includes the case $m = 1$, which gives the convex polygon which is not knotted. The number of knots is therefore $\frac{1}{2}\phi(n) - 1$. For the dodecagon ($n = 12$), $\phi(12) = 4$ and $\frac{1}{2}\phi(12) - 1 = 1$; there is therefore

only one knot, given by the scheme in Fig. 2. This knot is shown in Fig. 3. We notice that while the mid-line of the strip follows the star-polygon for $n=5$, the edges of the strip form the degenerate star-polygons with $m=4$ and $m=6$; i.e. they form the four triangles and the six diameters. This will be true in general: if we adopt Coxeter's notation $\left\{\frac{n}{m}\right\}$ for the star-n-gon in which each edge joins vertices m steps apart, then we may say that to every non-degenerate $\left\{\frac{n}{m}\right\}$ (n, m coprime) there is a knot with this as mid-line, and the edges of the knot are the star-polygons $\left\{\frac{n}{m-1}\right\}$ and $\left\{\frac{n}{m+1}\right\}$, which may or may not be degenerate.

We now enquire whether these knots have central holes. There are two different cases. (i) n *even*. The edges of the strip can be made to pass through the centre when $m=\frac{1}{2}n-1$. A single-strip knot of this type (a *rosette*) is possible if m is prime to n. The only possible common factor of m and n is 2, and this will appear only when $\frac{1}{2}n$ is odd. In this case a single strip will only visit half the edges (Fig. 4*a*, $n=10$) and two strips will be needed to complete the knot. (Fig. 4*b* shows the single-strip knot with $m=\frac{1}{2}n-2$). (ii) n *odd*. The mid-lines are nearest to the centre when $m=\frac{1}{2}(n-1)$ which is always prime to n. Since $m+1$ is greater than $\frac{1}{2}n$, the edges of the strips overlap the centre in this case. To sum up, a knot without a central hole is possible in all cases except when n is of the form $4k+2$. In this case a two-strip knot without a central hole is possible, as indeed is the case for all even n.

The theory of numbers tells us that $\phi(n)=n\prod_p(1-1/p)$, the product being taken over all prime factors of n. The first few values of $\phi(n)$ are shown in the table.

n	3	4	5	6	7	8	9	10	11	12	13	14	15	16
$\phi(n)$	2	2	4	2	6	4	6	4	10	4	12	6	8	8

When $\phi(n)=2$, $\frac{1}{2}\phi(n)-1=0$, and no knots are possible. This is the case when $n=3$, 4, or 6. Open loops are possible in these three cases (Fig. 5), but they are not strictly knots. The loop for $n=4$ is however quite well locked together.

The mystery of the regular hexagon is now solved. The two-strip knot which has to be used in this case is the familiar "grannie" (Fig. 6).

For those who are interested to try to make one or two of the single-strip knots it should suffice to say that the first step is to fold the strip back on itself at an angle of π/n, if n is odd, $2\pi/n$ if $n=4k$, and $4\pi/n$ if $n=4k+2$. When n is even, or when a "hollow" knot

is being tied, each fold will reverse the strip so that alternate sides show. If a paper which is coloured differently on the two sides is used a rosette with alternate colours appears. When n is odd, however, the folds cannot be reversed in the closed knot, because, as already noted, the strips overlap the centre and the same side shows all the time. (See Fig. 7). If tracing paper is used for the strips beautiful star-polygons can be seen when they are held up to the light.

I am indebted to Dr. H. M. Cundy for suggestions about the theory of star polygons.

Chatham Technical School J.K.B.

JOHN SMITH'S PROBLEM

BY E. R. LOVE

I make here two comments on Chaundy and Bullard's interesting article with the above title (Math. Gaz. no. 350, vol. XLIV (1960), 253–260).

One comment refers to their final sentence, in which they remark that they have not found a *direct* proof that

$$\int_n^\infty \frac{e^{-x}x^{n-1}}{(n-1)!}\,dx \to \frac{1}{2} \quad \text{as } n \to \infty.$$

The proof of Stirling's formula given in Goursat's *Cours d'Analyse*, vol. I, p. 279, is easily adapted to provide this conclusion. This method of proof is direct in that it deals directly with the integral in question; but it is not without subtlety. The chief thing is the substitution $e^{-x}x^n = n^n e^{-n} e^{-t^2}$, which leads to two formulae

$$\int_n^\infty e^{-x}x^n\,dx = \tfrac{1}{2}n^n e^{-n}\sqrt{2\pi n}\,\{I + O(1/\sqrt{n})\} = \int_0^n e^{-x}x^n\,dx,$$

where I is a positive constant whose value is not required, although in fact

$$I = \frac{2}{\sqrt{\pi}}\int_0^\infty e^{-t^2}\,dt = 1.$$

My other comment refers to Chaundy and Bullard's property (ii) (p. 255). I give an independent proof of this by expressing the probability function involved by an incomplete beta-function. Such an expression is mentioned in Levy and Roth's "Theory of Probability", p. 7, as having been known in the eighteenth century; but in my ignorance I had obtained it by using Taylor's theorem with remainder to provide an expression for Chaundy and Bullard's $S^n(q, x)$ (p. 256, equation (1)) as follows:

$$1+qx+\frac{q(q+1)}{2!}x^2+\ldots+\frac{(q+n-2)!}{(q-1)!(n-1)!}x^{n-1}$$

$$=(1-x)^{-q}-\frac{(q+n-1)!}{(q-1)!(n-1)!}\int_0^x\frac{(x-u)^{n-1}}{(1-u)^{q+n}}du$$

So much for the origins of the proof. We now proceed to give the proof in a self-contained form, using Chaundy and Bullard's notation (so far as it is needed) with only one exception.

Let $g(s, t, n)$ be the probability, for a die with s sides (or faces) all equally likely, that in t throws a specified side turns up less than n times. We aim to prove *that $g(s, sn, n)$ increases with s for fixed n.*

It is implicit that s, t, n are integers such that

$$s>1, \quad t\geqslant n, \quad n>0.$$

Putting $x=1/s$, so that $0<x<1$, and $y=1-x$, we have

$$g(s,t,n)=\sum_{r=0}^{n-1}\binom{t}{r}x^r y^{t-r} \tag{1}$$

by the binomial distribution, and also

$$g(s,t,n)=\binom{t}{n}n\int_x^1 v^{n-1}(1-v)^{t-n}\,dv \tag{2}$$

The latter may be verified by repeated integration by parts, integrating the power of $1-v$ each time; so there is no need to expand my earlier remarks on obtaining it from (1) in a pioneering (!) fashion.

From (1), $\quad g(s,t+1,n)-g(s,t,n)$

$$=\sum_{r=0}^{n-1}\left\{\binom{t+1}{r}y-\binom{t}{r}\right\}x^r y^{t-r}$$

$$=\sum_{r=0}^{n-1}\left\{\binom{t}{r-1}y-\binom{t}{r}x\right\}x^r y^{t-r} \qquad \text{writing } \binom{t}{-1}=0,$$

$$=x\left\{\sum_{r=0}^{n-1}\binom{t}{r-1}x^{r-1}y^{t-r+1}-\sum_{r=0}^{n-1}\binom{t}{r}x^r y^{t-r}\right\}$$

$$=-\binom{t}{n-1}x^n y^{t-n+1}. \tag{3}$$

Using (2) as definition of $g(s, t, n)$ for all $s>1$, but restricting t and n as before. Chaundy and Bullard's property (ii) will follow immediately if we prove that

$$g\left(s+\frac{1}{n}, \left(s+\frac{1}{n}\right)n, n\right)-g(s, sn, n) \tag{4}$$

is positive, for all $s>1$ and all integers $n>0$. Expression (4) is

$$g(s+1/n, sn+1, n)-g(s, sn+1, n)+g(s, sn+1, n)-g(s, sn, n)$$

$$=\binom{sn+1}{n} n \int_{1/(s+1/n)}^{1/s} v^{n-1}(1-v)^{sn-n+1}\, dv-\binom{sn}{n-1} x^n(1-x)^{sn-n+1} \tag{5}$$

using (2) and (3). Now the integrand is decreasing in the range of integration, because its derivative with respect to v has the sign of

$$(n-1)(1-v)-(sn-n+1)v$$
$$=(n-1)-snv\leqslant n-1-\frac{sn^2}{sn+1}=\frac{n}{sn+1}-1<\frac{1}{s}-1<0.$$

Thus (5) exceeds

$$\binom{sn}{n-1}\left\{(sn+1)\left(\frac{1}{s}-\frac{n}{sn+1}\right)\left(\frac{1}{s}\right)^{n-1}\left(1-\frac{1}{s}\right)^{sn-n+1}-x^n(1-x)^{sn-n+1}\right\}$$

$$=\binom{sn}{n-1}\left\{\left(\frac{1}{s}\right)^n\left(1-\frac{1}{s}\right)^{sn-n+1}-x^n(1-x)^{sn-n+1}\right\}=0.$$

So (4) is positive, as required.

Identity (3) also leads easily to a proof of Chaundy and Bullard's property (i); but this probably amounts to scarcely more than a re-presentation of their proof.

University of Melbourne E.R.L.

THE MATHEMATICAL DESCRIPTION OF NATURE*

By E. H. Sondheimer

Important changes are taking place at Westfield College. A new Science Faculty is being created and—although the College is, indeed, no stranger to scientific activity—the scope of the future effort in this field will be on a much larger scale than at any time in the past. I speak to you today as one of the five new professors who have recently joined the College as a result of the expansion; but my position as professor of Mathematics is not quite the same as that of the other four science professors. I am, in a sense, not such a novelty—and this perhaps is why I have been asked to be the first to give my Inaugural Lecture—since my department already exists. In fact mathematics has been taught at the College continuously from its earliest days in the 1880's, and I should like to begin by paying tribute to the devoted work of my predecessor, Miss G. K. Stanley. Originally a Westfield student, she received her postgraduate mathematical education in Oxford at the feet of the great Hardy, and returned to Westfield College in 1931 to take over the direction of the Mathematics Department from Miss Whitby, who had held office for the preceding 36 years. Not only has Miss Stanley guided the department with a sure hand during a period which has seen profound changes in the teaching of mathematics in London, but she has also given invaluable service direct to the University as secretary of the Board of Studies in Mathematics for the past 16 years. We are very glad indeed to know that she is still with us to give us the benefit of her knowledge and experience in the development that lies ahead.

What *is* the status, as a University discipline, of mathematics, which is at present cultivated at this College alone among the sciences? In fact, *is* mathematics a science? She has in the past been called both the queen and the handmaiden of the sciences—so that her sex at least seems to be known, if not her social status—and her presence in a women's College seems natural enough. But let us try to look into this a little more deeply. It is not much use going to the dictionary: the entry there—as usual—begs the question. If we look at the traditional subdivision of University disciplines into faculties, of arts, science, engineering, and so on, then—while we have no doubt to which faculty, say, zoology on the one hand and English on the other should belong, I find that I have been assigned by the University to the Faculties of both Arts *and* Science (and I suppose I am lucky to have escaped Engineering!)—so we are not much further. If we go into the College library, we

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find books on mechanics, say, or electromagnetic theory, amongst the mathematics books, and the same books turning up again (if the library can afford them) amongst the physics books. So mathematicians indubitably seem to need science books. But ambiguity returns when we discover that, at the end of his (or her) career as a London mathematics student, the successful graduand emerges sometimes as a Bachelor of Science, sometimes as a Bachelor of Arts: our final examination is, at one and the same time, the B.A. Honours and the B.Sc. Special examination in mathematics. Clearly there is something peculiar about the status of mathematics—and to clarify the question we must have a look at what mathematics consists of and what it is that mathematicians try to do. Well, in a recent popular article it is stated by my colleague Professor Rogers that the main branches of mathematics are algebra, analysis, geometry, the theory of numbers and topology. Now, Miss Stanley certainly qualifies as a mathematician according to this: her field is the theory of numbers. But for me Professor Rogers's list is rather disquieting, since I have made no contributions at all to any of the subjects mentioned by him; on hearing this, you may be beginning to wonder whether the University of London has not made a terrible mistake. So perhaps I should try to explain how an appointment like mine is possible. It is possible because I can call myself an *applied* mathematician, whereas the topics in Professor Rogers's list must, by contrast, be referred to as belonging to *pure* mathematics. (He did mention that there are branches of mathematics—analysis in particular—which have useful applications, but he nevertheless identifies mathematics as such with pure mathematics exclusively.) It is the meaning and significance of this distinction between pure and applied mathematics that I wish to examine today—and although what I shall say will not be in any way original or profound, I think that the question does repay pondering a little from time to time.

So let us add such "applied" subjects as mechanics, hydrodynamics, electromagnetism, relativity and quantum theory to our list, leaving open (for the moment) the status of these topics as mathematical subjects, and let us admit that applied mathematics —and applied mathematicians—also have their place in the scheme of things. In fact we are rather careful, here in London, to keep a more or less exact balance between the pure and the applied sides, with neither predominating too much: thus the part of our Final examination that is common to all students consists of precisely two papers in pure mathematics, two in applied, and one in an equal mixture of both. At the same time, it is unfortunately true that pure and applied mathematicians often have very little to say to each other. We run separate groups of postgraduate lectures in

pure and in applied mathematics; we often have readerships and chairs, reserved exclusively to either pure or applied mathematics; and in some of our Universities we go so far as to have separate departments of pure mathematics and applied mathematics. Already at school (University College School, not far from here) I was taught pure mathematics by one master, applied mathematics by another (and very ably in both cases, I might add).

Now one might try to dismiss, with some justice, this gap, this separation between the two kinds of mathematics as just another unavoidable consequence of the huge increase in specialization that has occurred in mathematics, with the modern expansion of the subject, just as in most other branches of learning. But there is more to it than that: after all, even an algebraist nowadays may find it just as hard (perhaps harder) to talk to a pure mathematician specializing in certain branches of analysis as to an applied mathematician. The fundamental distinction (perhaps familiar to most of you) can be briefly stated: whilst applied mathematics uses mathematical methods to study and describe and formulate theories of observable phenomena in the external world, pure mathematics is essentially just pure logic: it starts with unspecified abstract objects (in geometry they may, for convenience, be called points, lines, etc.—but what we choose to *call* them is really quite immaterial); these abstract objects satisfy certain axioms, and pure mathematics proceeds to obtain theorems which can be logically deduced from the axioms by the method of mathematical proof. How the pure mathematician chooses his axioms, and why he regards certain axioms as being particularly important, valuable and fruitful, is an interesting question, with no simple answer, which we shall touch upon again later; the axioms may indeed be suggested by certain regularities observed in nature, but officially the test of practical experience is not supposed to have anything to do with the matter. Applied mathematics, on the other hand, is essentially pure physics: and the aims, methods and outlook of the applied mathematician are (or should be) basically no different from those of the theoretical physicist, natural philosopher, or whatever you like to call him. He uses the inductive approach, ultimately based on observation of the external world, introduced 300-odd years ago by Galileo and Newton in the form in which it has survived, essentially unchanged in spirit, to the present day. It is important to emphasize here that—whatever applied mathematics *is*—it is *not* to be regarded as mere practical, utilitarian, engineering—in short *applicable*— mathematics, as a mere collection of special tricks and techniques for solving bigger and better equations, for doing more and more complicated sums. Such activities are by no means to be despised—they often illuminate problems of great practical impor-

tance—but I submit that they should have no place (or at best a very subsidiary place) amongst the activities of a University mathematics department.

It is natural to argue from what has been said (and I have some sympathy with the argument) that the concept of applied mathematics as a distinct discipline should be abolished, that the term is a misnomer and that applied mathematicians should be officially legitimized as physicists. (I am now thinking primarily of University undergraduate teaching in mathematics.) This view of the matter is of course implicit in Professor Rogers's list; it is also, by and large, the view taken in the academic structure of Universities in countries such as the United States, France and Germany, where a student, if he wants to study mathematics, studies it essentially in the Rogers sense; and it is clearly reflected in the selection of subjects discussed at Congresses of Mathematicians. This view—the recognition that pure mathematics and applied mathematics differ fundamentally in philosophy and in outlook, and that academic curricula should be reconstructed so as to take formal notice of this fact—has some powerful and persuasive advocates in this country. (It has, incidentally, also some important implications for the teaching of physics, but it is not my business to discuss these today.) The view has the great (if somewhat un-English) advantages of consistency and logical tidiness; I have (as I have said) considerable sympathy with it; and yet I think it is mistaken. After all, in spite of all that has been said, the very remarkable and (in some ways) really rather mysterious fact is that pure mathematics and applied mathematics (or mathematics and physics, if you will), though logically distinct, have in fact grown up through the ages in intimate association; they have constantly cross-fertilized and drawn mutual inspiration from each other; and even today they still have, in my view, enough to say to each other and to learn from each other to make one feel that the fruitful union of the two subjects, hallowed by 4,000 or so years of tradition, should not now be forced to end in a divorce on grounds of incompatibility. In fact, this ultimately rather mysterious interconnection is, to my mind, one of the most fascinating aspects of the subject—that the (apparently) purely abstract and theoretical *jeux d'esprit* of the pure mathematician have had, and do have, such a profound bearing on our empirical awareness of the external universe. Further exploration of this matter is extremely interesting but quickly involves one in deep problems concerning the nature of physical reality and the extent to which all knowledge is ultimately subjective. I do not propose to venture into such deep philosophical waters where I feel entirely out of my depth. The remarkable fact which I want to emphasize, however, is that the recognition of the true relation (or

what we consider today to be the true relation) between mathematics and physics is of comparatively recent origin. In the early days of the subject (and indeed right down to the 19th century) the close relation I have referred to between pure and applied mathematics was so intimate that the real distinction between the two disciplines was not clearly recognized at all. It is instructive to spend a little time delving into history at this point. If we go back to the beginning, there is little doubt that in its earliest origins mathematics was thoroughly "applied"; the first rudimentary mathematical steps taken by primitive civilizations must have been prompted directly by practical needs. Primitive methods of barter depended upon some ability to count (the primitive method of counting is recalled by our own word *digit*, which denotes a finger or a toe as well as the numbers 1, 2, 3 . . .). Primitive geometric concepts arose from observation of figures formed by physical objects: the concept of angle, for example, probably first came from observation of the angles formed at elbows and knees (we still speak of the arms of a right-angled triangle); the word geometry itself denotes the science of earth measurement, a thoroughly "applied" occupation. But there is also little doubt that, already in ancient Egypt and Babylonia, mathematical activity was not by any means exclusively utilitarian, confined to the solution of practical problems; the remarkable arithmetic properties of numbers and the geometric properties of regular figures both had their parts to play in all kinds of artistic activity, in architecture, in religious mysticism, magic and in astrology. However, the mathematics practised in the two ancient civilizations remained empirical, confined to the development of special techniques for solving numerous special problems, and failed to produce any general development of the subject based upon general principles of wide validity. As we all know, the creation of the modern mathematical spirit took place in Greece, and was indeed one of the supreme achievements of the Greece of the classical period. It was the Greek philosophers who first insisted that mathematical conclusions were to be reached by deductive reasoning only and who first introduced the abstract concepts—abstractions from experience—that form the raw material of mathematics up to the present day. In their emphasis on shape and form rather than on measurement and calculation, on geometry rather than on arithmetic, trigonometry and algebra, the Greeks were indeed the purest of pure mathematicians, and in that most remarkable culmination of Greek mathematics—the Elements of Euclid—they produced the most famous (though perhaps not most universally popular) textbook in history. The subsequent history of geometry is particularly illuminating and relevant to our present theme. Euclid's geometry contains many of the essential in-

gredients of modern pure mathematics. It starts by defining (in a way that would admittedly no longer be acceptable today) the fundamental concepts (points, straight lines, planes, circles and the like) with which geometry has to deal; a straight line, for example, is an idealized version of a string stretched tightly between its endpoints, with such attributes as the colour of the string, its thickness and the material of which it is made excluded from consideration. A number of axioms are then introduced as basic premisses for the theory, and all the manifold propositions and theorems of Euclidean geometry, (which most of us have heard about in our youth but have now forgotten) are then deduced from the axioms by strict logical proof. (It is a pity, incidentally, that, while Euclid's geometry is so rapidly disappearing from the mathematics curriculum of our grammar schools, no discipline of comparable elegance and rigour has been found appropriate to take its place there.) What can we say about the way in which the axioms of Euclid's geometry were selected? In the first place we note that they were small in number (ten, to be precise), logically independent of each other (in the sense that none of the axioms could be deduced as a logical consequence of any of the others), simple and readily acceptable to the intuition. (For example: "it shall be possible to draw a straight line joining any two points"; "the whole is greater than any of its parts".) But, while simple, Euclid's axioms were far from being trite superficialities, since they yielded an immensely rich harvest of profound and far from obvious theorems, all deduced from the axioms by step-by-step logical deduction and therefore, in a sense, already implicit in the axioms themselves. All these criteria—of economy in number, simplicity, intuitive appear and fruitfulness in their consequences—are just the ones we look for in a set of axioms for a modern mathematical discipline. But are the axioms *true*—in other words, does Euclidean geometry describe the properties of the real physical space of our sense experience? For 2,000 years after Euclid men had not the slightest doubt in the matter: the results of Euclidean geometry, derived by strict logic from "obviously true" premisses, agreed perfectly in every instance with observation of the physical world. In particular, Euclidean geometry had its due part to play in the great successes of Newtonian physics, and its complete identification with the properties of the physical world received its formal philosophical blessing at the hands of Kant. By 1800 it seemed clearer than ever before that Euclid's geometry was truth, gospel truth—in Professor Morris Kline's words, educated people were far more likely to swear by the theorems of Euclid than by any statement in the Bible. So it was indeed a great revolution in human thought when it was realized, early in the 19th century, that it was possible to construct other

systems of geometry, based on axioms different from Euclid's, which were just as logically self-consistent and free from contradictions as Euclidean geometry itself, and which could therefore coexist peacefully with Euclid as valid mathematical disciplines. For long before this, some people had in fact felt a little unhappy about some of Euclid's axioms—in particular the one that postulates (in effect) that it is possible to draw just one (and only one) straight line through a given point parallel to a given straight line. The trouble with this axiom is that it is rather less intuitively obvious—as a statement of the properties of physical space—than most of the others. To show that two lines are parallel, in other words that they never intersect, it is necessary to extend them indefinitely into remote regions of space; and, since man is located within such a tiny portion of the universe, his intuitive, " commonsense ", notions of space are limited to direct experience of distances of the order of a mere few hundred miles, say, measured north, south, east or west and (at least until very recently) still much smaller distances when measured vertically up or down. Euclidean geometry was here faced with the problem of the infinite, which worried the Greeks very much and with which they never managed to cope (it still bothers us today, of course); and Euclid himself—without, to be sure, doubting the truth of his parallel axiom—seems to have felt somewhat uneasy about it since he tried to prove as many theorems as he could without its use. Many attempts were made, during the 17th and 18th centuries, to clarify the air of mystery that surrounded the bothersome axiom. People tried either to deduce it from the other axioms of Euclid's geometry (and thus to convert its status from that of an axiom into that of a theorem); or to find some more acceptable substitute; or to show that the adoption of any other axiom in place of the parallel axiom necessarily led to contradictory theorems and was therefore inadmissible. All such attempts ended in failure, but for many years the true implications of this failure were not recognized—the habits of thought engendered by 2000 years of Euclidean geometry were too persistent. There were those, in particular, in the 18th century who tried to see what happens when, instead of Euclid's parallel axiom, one postulates that there are at least *two* lines through a point parallel to a given straight line. On the basis of this a large number of theorems were deduced which, remarkably enough, involved no logical contradictions of any kind; but at that time people refused to accept the evidence of their own logic and concluded that the strange and novel theorems they had deduced were too absurd to make sense, so that Euclid's geometry must be the only possible one after all. The best part of another century had to elapse before—with the work of Gauss, Bolyai and Lobatchevsky (the last the hero of

Tom Lehrer's immortal song)—the real significance of these results was discovered and non-Euclidean geometry was born; and yet more time had to elapse before, around the middle of the 19th century, the mathematical world in general woke up to the full import of the revolution that had taken place. If the birth of modern pure mathematics is to be regarded as dating from any particular period, it is surely this one: for the discovery of non-Euclidean geometry set mathematics free to range wherever it liked, to invent new systems of axioms and thence to develop entirely new branches of mathematics at will, uninhibited by the constricting requirements of mere common sense and intuitive appeal; the liberation thus achieved has indeed given rise to tremendous developments in mathematics during the past 100 years—and the pace shows no signs of diminishing today.

But—now that we can have many geometries, all equally valid as consistent mathematical disciplines—what of the description of the real, external world? The answer is that we must now distinguish between mathematical space and physical space, between (in Einstein's words) "pure axiomatic" geometry on the one hand, and "practical" geometry on the other; in the former the pure mathematician is entirely free to invent and describe spaces of his own making, but the latter is a branch of applied mathematics, and the properties of physical space—the space of the external world—are to be settled by appeal to observation. To the pure mathematician a straight line is an undefined concept, characterized *exclusively* by the axioms appertaining to it: it is this recognition that pure mathematics must necessarily deal in entities inherently incapable of definition that distinguishes the modern approach from that adopted by Euclid. In applied mathematics a straight line has to be defined in terms of, say, the position of a tightly stretched string, the path of a light ray, or the shortest distance between two localities; and the laws that apply to geometric entities defined in this way can be discovered by experiment. In fact, if we choose to define a straight line as the shortest distance between two points on the surface of the earth (assumed spherical)—a natural enough definition—we are led to a geometry (discovered by Riemann) that is not Euclidean: the straight lines in this geometry are of finite length (equal to the length of the earth's equator), and there are no parallel lines at all since *any* two straight lines must always intersect. It is only the somewhat accidental fact that the circumference of the earth is so large compared with the length of a day's walk—or even the length of the journeys undertaken by the ancients—that delayed the discovery of non-Euclidean geometry for so long.

The final chapter in the story takes us into the present century. With the discovery of non-Euclidean geometry it had to be recog-

nized that there was no longer any *a priori* guarantee as to the Euclidean character of physical space; but in spite of this no one seriously doubted that physical space was indeed Euclidean, that, although pure mathematicians might play with new-fangled kinds of geometry, figures in real space continued to obey the theorems of the geometry of the ancients. As we have seen, the matter was one to be decided by appeal to observation, and observation invariably ruled in favour of Euclid. Or, to be more precise: no decision was possible on the basis of terrestrial experiments, since the predictions of Euclidean and non-Euclidean geometry agree with each other to an accuracy far greater than the limits of experimental error. (Gauss himself tried to investigate the matter by measuring the angles of a large triangle formed by observers stationed on three mountain peaks. Non-Euclidean geometry predicts an angle-sum different from 180 degrees: but there was no hope of measuring the angles with sufficient precision to detect the minute discrepancies in question.) The cosmic scale was needed to bring non-Euclidean geometry unequivocally into physics, and it was Einstein's theory of gravitation—probably the greatest intellectual achievement of the 20th century—that finally established non-Euclidean geometry as a valid description of the physical world. We have no time today to discuss in any detail the profound elegance and beauty of Einstein's work: we may summarize it (quite inadequately) by remarking that, in Einstein's theory, the geometric character of space (or, more precisely, of the four-dimensional space-time world) is supposed to depart from the Euclidean in a way that depends upon the distribution of matter in the universe. If we then suppose that particles will move—as one would naturally expect them to!—along the "straight lines" in this new geometry (technically known as geodesics), we find that the predicted paths agree with observation: the effects of gravitation have been incorporated into the geometry of the universe. We emphasize again that the final decision as to the type of geometry that is to be used to describe the world ultimately rests upon observation; and—whilst the ultimate validity of Einstein's work is still a somewhat controversial question—the important point is that we are by now quite reconciled to the possibility of a non-Euclidean physical world—so that both in physics and in pure mathematics the "common-sense" belief in the universal validity of Euclid's geometry—so firmly held a mere 160 years ago—has by now been well and truly abandoned.

As another illuminating example of the intimate union of pure and applied mathematics, and of a very fundamental development that took place in the 19th century, we may cite the history of the infinitesimal calculus, the discipline that lies at the base of virtually all of modern mathematics. The calculus is concerned with the

notions of limits and of rates of change which were indispensable for the development of a satisfactory theory of motion and thus for the tremendous development of Newtonian dynamics, in the 17th and 18th centuries, at the hands of Newton himself and of his formidable successors such as Laplace and Lagrange. But it is remarkable that, try as they might (and they tried hard!), the men who developed the calculus, and who used it to reap an immense harvest of mathematical discoveries, were quite unable to give any clear and rigorous formulation of the subject; in the words of Courant and Robbins, the Greek ideal of axiomatic crystallization and systematic deduction disappeared in a veritable orgy of intuitive guesswork and of cogent reasoning interwoven with nonsensical mysticism. Fortunately the pioneers were extremely able men whose instinct and intuition kept them from going far wrong. But a rigorous formulation of the subject proved so elusive that people began to think that no such formulation could ever be found. It *was* found of course—eventually—and it was once again left to the mathematicians of the 19th century to achieve the deepened insight into the nature of mathematical activity that was necessary for the task. Thus the 19th century witnessed both the liberation of mathematics from the predominance of Euclidean geometry as the only valid system of geometry, and a return to (and indeed beyond) the Euclidean ideal of precision and rigorous proof.

What of the present day? I think it is true to say that, in pure mathematics, the critical spirit and preoccupation with the logical foundations of the subject that characterized the 19th century is still predominant. Indeed a whole branch of modern mathematics —mathematical logic—occupies itself specifically with the problem of finding a satisfactory logical basis for the whole of mathematics. In spite of all modern advances, this ultimate goal has proved singularly elusive, and mathematicians do not even agree with each other about the way the problem ought to be formulated. The school of " formalists ", inspired by the great Hilbert, insists that freedom from contradiction is the only thing that matters. Their task, therefore, is to find a set of basic postulates from which all of mathematics can be deduced by formal logical procedure, and to prove that such a set of postulates can never possibly lead to a contradiction. The programme sounds reasonable enough, but it has not so far been carried out and, what is more disturbing, it even looks as if it is inherently incapable of *ever* being carried out. Another school—the " intuitionists "—declares that freedom from contradiction is not enough: one must actually be able to *construct* the objects that one talks about in mathematics. This demand again seems fair enough, and it disposes of many basic logical difficulties—but only at a very high price. For example, we can

never *construct* in its entirety the infinite decimal that represents the number π, and so strict intuitionism would have us conclude that π has no decimal expansion. In fact this attitude, when carried to its logical conclusion, compels us to throw overboard a great deal of present-day mathematics and—whether or not this is a reasonable demand—it is much too drastic a remedy for most mathematicians. Well—there are no final answers here, and perhaps we should wish that there never will be. At any rate it seems safe to conclude that it is extremely difficult to banish the intuitive element from mathematics. And indeed it must never be forgotten that in pure, as in applied mathematics, truly creative activity in practice by no means proceeds by strict logical deduction from a given set of axioms. True mathematical creation rests on inspiration and intuition, upon the creative use of the imagination, just as much as the composition of a work of music, and the formalistic presentation of results—imposed by the rules of the game—is really largely a pretence. Of course there are good reasons for this pretence—we cannot dispense with it, and let no one tell you otherwise—but pretence it is all the same, and—whilst every working mathematician knows all this well enough—we must take care in our teaching that the pretence is not mistaken for the reality; it is this misunderstanding that may easily make the subject appear sterile and incomprehensible. In fact it is important to realise that both in pure mathematics and in applied mathematics the two aspects—the "intuitive" and the "deductive"—both have their parts to play. In applied mathematics, to be sure, the rules of the game are looser, the intuitive element cannot be so tidily hidden away; we cannot reasonably demand, as we do in pure mathematics, that each new contribution be presented in strictly deductive fashion. But, once a branch of the subject has been fully established, we do try to bring out its logical coherence in just this way by developing it, like a branch of pure mathematics, from a fundamental set of axioms which are chosen so as to summarize some particular aspect of our experience of the external world. At the present time, for example, the special theory of relativity (dating from 1905) and non-relativistic quantum theory (dating from 1926) have been developed to the point where they are ripe for axiomatization. It is only at this stage that really satisfactory textbooks can be written on the subjects in question; the subjects have then become parts of standard knowledge, incorporated among the regular tools of the working scientist.

One could give innumerable further examples of the ways in which, in the history of mathematics, both ancient and modern, the pure and applied branches have mutually influenced each other in decisive fashion. Again and again the creation of a new branch

of theoretical science has been found to call for the use of mathematical tools that were already in existence—fashioned, often at a much earlier date, by pure mathematicians who had not the slightest inkling of the scientific (the " applied ") implications of their work. The example of non-Euclidean geometry has already been mentioned; to take both an earlier and a later example: 1500 years before Kepler used the ellipse to describe the motions of planets and Galileo the parabola to discuss the motion of projectiles, the Greek mathematicians had already fully explored the mathematical properties of the conic sections; and in modern theoretical physics, where the emphasis is more and more upon the exploration of order and structure, upon the investigation of fundamental symmetry properties of nature, much use is made of the mathematical apparatus of the theory of groups, a discipline developed in the 19th century as a branch of pure mathematics—and still taught strictly as such to our undergraduates today, even though a recent book on the theory of the elementary particles of nature devotes its first 90 pages to the study of the mathematics of the 4-dimensional orthogonal group. And the history of the calculus shows, conversely, how pure mathematics has been enriched by the imperative needs of theoretical science, in leading men to the bold creation of new kinds of mathematics without waiting for the blessing of orthodox pure mathematicians. There is a striking modern example of this: the delta function of Dirac, of fundamental importance in quantum theory, was for long regarded with horror by pure mathematicians since it did not conform in the least to any of the rules required of respectable functions in respectable mathematics, but it has now been thoroughly rehabilitated in the entirely proper theory of distributions and generalized functions invented some years ago by Laurent Schwartz. Had time allowed, I should have liked to tell you something of current investigations in quantum field theory—into the most fundamental questions in modern theoretical physics—which have raised problems, in subjects such as the theory of several complex variables, of considerable interest for the pure mathematician. Time does not allow, so let me just read you part of the summary of a recent paper in the Physical Review: " the problem of unstable particles in quantum field theory is treated as one of the interpretation of complex singularities appearing in the analytic continuation of scattering amplitudes into unphysical sheets of their Lorentz invariant variables." Evidently theoretical physics and pure mathematics are close partners still, but—alas—we have become so specialized that there are probably very few members of the audience who will have understood *all* the implications of the sentence which I have just read out, although in fact each of the terms employed must be familiar *either* to a

mathematician *or* to a physicist.

We are getting far too technical, but I hope that it has become clear that modern pure and applied mathematics have many trends in common. An increased degree of abstraction and sophistication is evident in both, so that, what used once to be thought of as simple plain common sense—say the universal absolute truth of Euclid's axioms, or the fact that electromagnetic phenomena must be explicable in mechanical terms—comes to be regarded in later periods as obvious prejudice. At the same time the vast extension of the range of enquiry has been coupled with a severe *restriction* of the field, in the philosophical sense. Our ultimate aims become much more modest: in physics we no longer ask, since Galileo's day, *why* things work, or what purpose they serve, and in pure mathematics we no longer look for the meaning of our objects as substantial things in themselves. The miracle is that this modesty has brought such rich rewards: the less ambitious we become in our ultimate philosophic aims, the greater by far becomes our power of analysis and depth of understanding. It is this emphasis upon the elimination of metaphysical concepts, common to pure and applied mathematics and, indeed, a dominant characteristic of modern scientific thought, that seems to me to form perhaps the strongest link between the two branches of the subject and makes it so valuable that both be taught together: one does not have to be an out-and-out logical positivist to feel that, if our teaching is to have any genuine regard for living realities, this philosophy must inevitably colour our approach.

This leads me, in conclusion, to some comments upon the teaching of applied mathematics. The study of nature in the 20th century has been dominated by the two great doctrines of the theory of relativity and of the quantum theory. These have taught us ways of dealing with distances on the cosmic scale and on the subatomic scale, and with velocities approaching the velocity of light: ways of thinking, therefore, that are of necessity far removed from the intuitive, common-sense world of our every-day experience which is summarized (for the most part) in the laws of Newtonian mechanics, so conspicuous in undergraduate applied mathematics. I am not going to argue that we should banish Newtonian dynamics from our syllabus: it is still a beautiful and vitally important subject (this goes without saying), the basic foundation of all that came later, but I *would* urge that it be taught as a living part of physics, with a proper emphasis upon those aspects that we now know to be the significant ones for modern times, and with a proper discussion of the limitations of the subject and a glimpse (at least) of the generalizations that have superseded it. In the age of nuclear energy these developments are important enough, in all truth, from

the strictly pragmatic point of view—and, if quantum mechanics can be taught to undergraduate physicists, why can it not be taught to mathematicians also?

To those who would urge that the modern theories are too abstract, too difficult to be grasped by the young, we would reply that Newtonian physics itself rests upon idealizations which, although now apparently so "obvious", themselves took many centuries to be clearly formulated, and which must themselves have been regarded at one time as being in violent conflict with the common-sense notions of the day. If we adopt the outlook I have in mind, we shall (for example) reduce our preoccupation with the complicated motions of rigid bodies—useful, of course, for designers of tops, flywheels and gyroscopes—but of little interest in modern theoretical physics where the very concept of a rigid body has virtually disappeared. Above all, we shall never regard applied mathematics as an exercise ground for mere tricks of calculation, for solving sterile problems that do not illuminate any physical principles. Admittedly, tricky problems in mechanics are ideal material for examination questions, but with regard to this I believe that what Hobson said in 1910 is still relevant: "I think the convenience of the examiner, and even precision in the results of examinations, ought unhesitatingly to be sacrificed when they are in conflict with the vastly more important interests of education. Of the many evils which our examination system has inflicted upon us, the central one has consisted in forcing our school and university teaching into moulds determined not by the true interests of education, but by the mechanical exigencies of the examination syllabus."

After all this, have I answered my initial question—is mathematics a science? Strictly speaking I have not—it is probably one of those questions that are inherently incapable of a strict yes or no answer. Perhaps in the last resort we shouldn't worry too much about it, and remember that when we talk about arts subjects, science subjects and so on, we are using a classification determined by man-made administrative convenience that should not be elevated into a fetish endowed with absolute significance. I have even heard rumours that our friends the historians—officially firm members of the arts camp—have been known to argue fiercely about precisely the same question: is history a science? I would prefer us to emphasize instead those aspects of our studies that are common to all of us in the academic world in our efforts to reach deeper understanding. I hope that this spirit will animate us in the Mathematics Department at Westfield College—mathematicians of all kinds—in our efforts for the welfare and progress of the new Science Faculty and, beyond that, for the intellectual life of the College as a whole and of the University to which we are proud to belong. E.H.S.

ON FERMAT'S LAST THEOREM

BY LOUIS LONG

In my note in the December 1960 Gazette I proved that if there is an odd prime p and numbers a, b, c prime to p such that

$$a^{2p}+b^{2p}=c^{2p} \tag{1}$$

then p necessarily has one of the forms $120k+1$, $120k+49$.

In the present note I extend the range of values of p for which equation (1) has no solution with a, b, c prime to p. My method is to find values of p for which one of the numbers a, b, c in equation (1) is necessarily divisible by 11.

Any odd prime p has one of the forms

$$10k \pm 1, \quad 10k \pm 3$$

(for numbers of the form $10k \pm 5$ are not prime). I showed in my previous note that (1) has no solution prime to p when p has the forms $5k \pm 2$, and so there remains to consider only values of p of the form $10k \pm 1$. I have obtained no results in the case $10k+1$ and I shall now consider the case $10k-1$. With $p=10k-1$ equation (1) takes the form

$$(a^2)^{10k-1}+(b^2)^{10k-1}=(c^2)^{10k-1}$$

and since, by Fermat's little theorem, $x^{10}\equiv 1 \pmod{11}$, for any x, we have

$$a^{-2}+b^{-2}\equiv c^{-2} \pmod{11}$$

that is

$$b^2c^2+c^2a^2-a^2b^2\equiv 0 \pmod{11} \tag{1.1}$$

The quadratic residues of 11 are

$$+1, \ -2, \ +3, \ +4, \ +5 \tag{1.2}$$

Because none of a, b, c is divisible by p, it follows as in my previous note that c^2-a^2, c^2-b^2, a^2+b^2 are all squares so that we may write

$$c^2-a^2=A^2 \tag{2}$$
$$c^2-b^2=B^2 \tag{3}$$
$$a^2+b^2=D^2 \tag{4}$$

Considering the quadratic residues to modulus 11, listed in (1.2), and writing h_r^2 for the remainder when h^2 is divided by 11, we see that to any value of c^2 correspond only two possible values of a_r^2. Let s, t be the two values of a_r^2 for a given c^2; it follows that the two values of b_r^2 are all s and t. If a_r^2 and b_r^2 have the same value, then by (4) we have $2a^2\equiv D^2 \pmod{11}$, which is impossible since 2 is not a

quadratic residue of 11. Therefore $a_r^2 = s$ and $b_r^2 = t$ (or *vice-versa*) and so

$$a^2 + b^2 = c^2 \pmod{11} \tag{5}$$

Writing (1.1) in the form

$$c^2(a^2 + b^2) = a^2b^2 \pmod{11} \tag{6}$$

it follows that

$$c^4 = a^2b^2 \pmod{11} \tag{7}$$

Similarly

$$b^4 = -a^2c^2 \pmod{11}, \tag{8}$$
$$a^4 = -b^2c^2 \pmod{11} \tag{9}$$

From (7), (8), (9) we obtain

$$c^4 = a^4 + b^4 \pmod{11} \tag{10}$$

and from (5)

$$c^4 = a^4 + b^4 + 2a^2b^2 \pmod{11} \tag{10.1}$$

whence

$$2a^2b^2 = 0 \pmod{11}$$

contradicting the hypothesis that neither a nor b is divisible by 11. Thus we may suppose that b is divisible by 11.

Hence the equation (1)

$$a^2 = b^2 \pmod{11}$$

that is, $c^2 - a^2$ is divisible by 11.

But

$$c^{2p} - a^{2p} = (c^2 - a^2)R$$

since $(c^2)^p - (a^2)^p$ is divisible by $c^2 - a^2$, and

$$\begin{aligned} R &= c^{2(p-1)} + c^{2(p-2)}a^2 + \ldots + a^{2(p-1)} \\ &= c^{2(p-1)} - a^{2(p-1)} + a^2(a^{2(p-2)} - a^{2(p-2)}) + \ldots + p \,.\, a^{2(p-1)} \\ &= 11s + p \,.\, a^{2(p-1)} \end{aligned}$$

Since R is a square, $p \,.\, a^{2(p-1)}$ is a quadratic residue of 11 and therefore p itself is a quadratic residue of 11.

Thus we have arrived at the following conclusion.

If p is a prime of the form $10k - 1$ then equation (1) has no solution a, b, c prime to p if p is a quadratic non-residue of 11.

From my previous note we know that there is no solution of (1) prime to p unless p has one of the forms

$$120l+1, \quad 120l+49,$$

only the second of which is of the form $10k-1$. It remains only to see which of the numbers $120l+49$ is a quadratic non-residue of 11. Since $120l+49=121l+44-l+5$, and since the non-residues of 11 are

$$-1, \ +2, \ -3, \ -4, \ -5,$$

we have

$$l-5=1, \ -2, 3, 4, 5 \pmod{11}$$

and so for the following values of p

$$120(11m+6)+49 = 1320n-551, \quad (n=m+1)$$
$$120(11m+3)+49 = 1320n+409, \quad (n=m)$$
$$120(11m+8)+49 = 1320n-311, \quad (n=m+1)$$
$$120(11m+9)+49 = 1320n-191, \quad (n=m+1)$$
$$120(11m+10)+49 = 1320n+71 \ , \quad (n=m+1)$$

equation (1) has no solution with a, b, c prime to p.

These values are all different from those already found in my previous note.

The method we have used for the prime 11 may be used successfully for any other prime, although we do not always obtain new values of p in this way. It is perhaps worth remarking, too, that the proof in my previous note which showed that the equation

$$a^{2p}+b^{2p}=c^{2p}$$

has no solution in integers prime to p when p has either of the forms $5m\pm2$, is valid whether p is prime or not and so the equation

$$a^n+b^n=c^n$$

has no solution with a, b, c prime to n when the terminal digit of n is 4 or 6.

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MATHEMATICAL NOTES

2974. The year 1961

This year will be unique in our lifetime for, of the years of this century, only 1961 is unaltered by being written upside down. This last happened in 1881 and will not occur again till 6009. How rare is such an event? Let us call a number *invertible* if it is a positive integer which is unaltered by being written upside down. Then the above question can be conveniently restated as: how many numbers less than 10^k are invertible?

There are 2 invertible numbers less than 10: 1 and 8. An invertible number between 10 and 100 is either an invertible digit repeated or a pair of digits each of which is the other written upside down, so there are 4 such numbers: 11, 69, 88, and 96. The invertible numbers with three digits can be found by putting 0, 1 or 8 in the middle of each of these numbers. There are therefore 12 invertible numbers between 100 and 1,000. Of course, no number of the form 10^n is invertible unless $n=0$.

To construct all invertible numbers with $n+2$ digits, where $n>1$ is even, it is only necessary to insert successively in the middle of all invertible numbers with n digits all invertible numbers with 2 digits and 00. Similarly, to construct all invertible numbers with $n+2$ digits where $n>1$ is odd, we insert successively on either side of the middle digit of each invertible number with n digits, the digits of each invertible number with 2 digits and the digits 00. Therefore, for $n>1$, there are five times as many invertible numbers with $n+2$ digits as invertible numbers with n digits. Consequently, for $n>1$, there are $4 \,.\, 5^{(n/2)-1}$ invertible numbers between 10^{n-1} and 10^n if n is even and $12 \,.\, 5^{[(n-1)/2]-1}$ if n is odd.

Let A_k be the number of invertible numbers less than 10^k. Then, by taking the sum of the numbers of invertible numbers with at most k digits, for $m>0$,

$$\begin{aligned} A_{2m} &= 2 + \sum_{n=1}^{m} 4 \,.\, 5^{n-1} + \sum_{n=1}^{m-1} 12 \,.\, 5^{n-1} \\ &= 2 + 4 \,.\, 5^{m-1} + 16 \,.\, \sum_{n=1}^{m-1} 5^{n-1} \\ &= 2 + 4 \,.\, 5^{m-1} + 4(5^{m-1} - 1) \\ &= 8 \,.\, 5^{m-1} - 2 \end{aligned}$$

and

$$A_{2m+1} = 2 + \sum_{n=1}^{m} 4 \,.\, 5^{n-1} + \sum_{n=1}^{m} 12 \,.\, 5^{n-1}$$

$$=2+16\sum_{n=1}^{m} 5^{n-1}$$
$$=2+4(5^m-1)$$
$$=4\,.\,5^m-2.$$

Therefore, there are $8\,.\,5^{(k-2)/2}-2$ invertible numbers less than 10^k if k is even and $4\,.\,5^{(k-1)/2}-2$ if k is odd. In particular, there will have been 198 years like 1961 by A.D. one million, 22 of which have already passed.

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2975. Paying a given sum in a given number of coins

Suppose we have a coinage system in which the names and values of the coins, in units of the value of the least coin, are, in ascending order, A_1 of value a_1 ($=1$), A_2 of value a_2, etc. For example if A_1 is a penny, A_2 a 3d. piece, A_3 a sixpence, etc. we have $a_1=1$, $a_2=3$, $a_3=6$, $a_4=12$, $a_5=24$, $a_6=30$.

Let $N_r(x)$ be the number of ways of paying x units, using the coins $A_1, A_2, \ldots A_r$, any number of each.

Then $N_r(x)$ is the sum of

(1) the number of ways in which A_r does not occur; i.e. $N_{r-1}(x)$

(2) the number of ways in which A_r occurs once; i.e. $N_{r-1}(x-a_r)$

(3) the number of ways in which A_r occurs twice; i.e. $N_{r-1}(x-2a_r)$

and so on.

Hence $N_r(x)=N_{r-1}(x)+N_{r-1}(x-a_r)+N_{r-1}(x-2a_r)+\ldots$

Writing $x-a_r$ for x,

$$N_r(x-a_r)=N_{r-1}(x-a_r)+N_{r-1}(x-2a_r)+\ldots,$$

so that

$$N_r(x)-N_r(x-a_r)=N_{r-1}(x)$$

and $N_1(x)=1$, $N_r(0)=1$.

Hence, using the coins mentioned above,

$$N_r(x)=N_{r-1}(x)+N_r(x-a_r)$$

with $a_1=1$, $a_2=3$, $a_3=6$, $a_4=12$, $a_5=24$, $a_6=30$.

It is possible to find an explicit formula for $N_r(x)$ when $r=1$, 2, 3 and 4, but the problem becomes intractable for $r>4$. It is easy to see that, if

$$\lambda=\left[\frac{x}{3}\right],\quad \mu=\left[\frac{x}{6}\right],$$

$$N_1(x)=1,\quad N_2(x)=1+\lambda,\quad N_3(x)=(\mu+1)(\lambda-\mu+1)$$

It can be proved that, if $x = 12p + R$,

$$N_4(x) = \tfrac{1}{3}(p+1)(p+2)\{3\lambda - 8p\}, \quad (R \geqslant 6) \qquad (a)$$

$$= \lambda(p+1)^2 - \tfrac{1}{3}(p+1)(8p^2+4p-3), \quad (0 \leqslant R < 6) \qquad (b)$$

The proof is as follows:

(1) Let $R \geqslant 6$.

$$\begin{aligned} N_4(x) &= N_3(x) + N_3(x-12) + N_3(x-24) + \ldots + N_3(x-12p) \\ &= N_2(x) + N_2(x-6) + N_2(x-12) + \ldots \\ &\qquad + N_2(x-12p) + N_2(x-12p-6) \\ &\qquad + N_2(x-12) + N_2(x-18) + \ldots \\ &\qquad + N_2(x-24) + N_2(x-36) + \ldots \end{aligned}$$

where $N_r(x-a) = 1$ if $x = a$
$= 0$ if $x < a$.

Hence

$$N_4(x) = 2 + \left\{\left[\frac{x}{3}\right] + \left[\frac{x-6}{3}\right]\right\} + 2\left\{2 + \left[\frac{x-12}{3}\right] + \left[\frac{x-18}{3}\right]\right\} + \ldots$$

$$\ldots + (p+1)\left\{2 + \left[\frac{x-12p}{3}\right]^* + \left[\frac{x-12p-6}{3}\right] \ldots\right\} \qquad (A)$$

In terms of $\lambda = \left[\dfrac{x}{3}\right]$ we get

$$\begin{aligned} N_4(x) &= \lambda(2+4+6+\ldots+2p+2) - 8\{2\,.\,1 + 3\,.\,2 + \ldots + (p+1)p\} \\ &= \lambda(p+1)(p+2) - 8\sum_{s=1}^{p}\{s(s+1)\} \\ &= \lambda(p+1)(p+2) - \tfrac{8}{3}p(p+1)(p+2). \end{aligned}$$

(2) Let $0 \leqslant R < 6$.

Now the series (A) ends with the * term, so that the last term of (A) is $(p+1)\{1+\lambda-4p\}$, and so

$$\begin{aligned} N_4(x) &= \lambda\{p(p+1)+p+1)\} - 8\sum_{1}^{p-1}\{s(s+1)\} - 4p(p+1) + p + 1 \\ &= \lambda(p+1)^2 - \tfrac{1}{3}(p+1)(8p^2+4p-3). \end{aligned}$$

The writers would be glad to know whether any reader can extend these results to values of $r > 4$.

Examples

(1) Let $x = 20$, then $p = 1$, $R = 8$, $\lambda = 6$ and using (a)

$$N_4(20) = \tfrac{1}{3}\,.\,2\,.\,3(18-8) = 20.$$

(2) Let $x = 24$, then $p = 2$, $R = 0$, $\lambda = 8$ and using (b)

$$N_4(24) = 8\,.\,3^2 - \tfrac{1}{3}\,.\,3(8\,.\,2^2 + 4\,.\,2 - 3) = 72 - 37 = 35$$

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2976. The Simson Line

1. This note provides an algebraic account of S. Zylbertrest's generalisation of the Simson line (M.G. No. 351 p. 30) with some further results.

A convention that all sub-indices are integers modulo n is observed throughout. The set of points (P_i) will be taken to determine the n-sided polygon $P_0P_1P_2...P_{n-1}$ unless another order is implied. The orthogonal projection of a point P onto the side P_kP_{k+1} of the polygon (P_i) is denoted by P_k^1. The polygon (P_i^1) is the first pedal of P w.r.t. (P_i) and the construction may be repeated to form successive pedals. Thus the rth pedal is the polygon (P_i^r) whose vertices are the orthogonal projections of P onto the sides of the $(r-1)$th pedal (P_i^{r-1}). Zylbertrest has shown that *the points P_i are concyclic with P if and only if the $(n-2)$th pedal of P w.r.t. (P_i) is a line.*

2. Suppose first that the points lie on a circle of unit radius whose centre will be taken as origin. With the sub-index convention define

$$T_k^r = \sum_k^{k+r-1} \tfrac{1}{2}(t_i - t - \pi)$$

where t_i is the parameter of P_i and t the parameter of P. It will be shown that the sides of the $(r-2)$th pedal of P w.r.t. (P_i) are

$$L_k^r \equiv x\cos(T_k^r + t) + y\sin(T_k^r + t) - \cos T_k^r + 2\prod_k^{k+r-1}\sin\tfrac{1}{2}(t_i - t) = 0$$

Suppose that this is so for some value of r. Then the coordinates of P_k^{r+1}, the foot of the perpendicular from P to the line $L_k^r = 0$, are given by

$$\xi_k^{r+1} = \cos t - 2\cos(T_k^r + t)\prod_k^{k+r-1}\sin\tfrac{1}{2}(t_i - t)$$

$$\eta_k^{r+1} = \sin t - 2\sin(T_k^r + t)\prod_k^{k+r-1}\sin\tfrac{1}{2}(t_i - t)$$

A cyclic permutation $t_i \to t_{i+1}$ in these coordinates will give ξ_{k+1}^{r+1} and η_{k+1}^{r+1}, the coordinates of P_{k+1}^{r+1}. Some tedious manipulation verifies that

$$\eta_k^{r+1} - \eta_k^{r+1} = C\,.\cos(T_k^{r+1} + t)$$

$$\xi_{k+1}^{r+1} - \xi_k^{r+1} = C\,.\sin(T_k^{r+1} + t)$$

and $$\xi_k^{r+1}\eta_{k+1}^{r+1} - \xi_{k+1}^{r+1}\eta_k^{r+1} = -C\cos T_k^{r+1} + C\,.\,2\prod_k^{k+r}\sin\tfrac{1}{2}(t_i - t)$$

where $$C = 2\sin\tfrac{1}{2}(t_{k+r} - t_k)\prod_{k+1}^{k+r-1}\sin\tfrac{1}{2}(t_i - t)$$

so that the equation of the side $P_k^{r+1}P_{k+1}^{r+1}$ is precisely $L_k^{r+1}=0$.

But

$$L_k^2 = x\cos(\tfrac{1}{2}t_k+\tfrac{1}{2}t_{k+1}-\pi)+y\sin(\tfrac{1}{2}t_k+\tfrac{1}{2}t_{k+1}-\pi)$$
$$-\cos(\tfrac{1}{2}t_k+\tfrac{1}{2}t_{k+1}+t-\pi)+2\sin\tfrac{1}{2}(t_k-t)\sin\tfrac{1}{2}(t_{k+1}-t)$$
$$=-x\cos\tfrac{1}{2}(t_k+t_{k+1})-y\sin\tfrac{1}{2}(t_k+t_{k+1})+\cos\tfrac{1}{2}(t_k-t_{k+1})$$

so that the lines $L_k^2=0$ are the sides P_kP_{k+1} of the original polygon. Hence it may be induced that the sides of the $(r-2)$th pedal are $L_k^r=0$ for all r. In particular, the cyclic permutation "swallows its tail" when $r=n$ so that $L_k^n=L_0^n$ for all k, i.e. the $(n-2)$th pedal is a line and the sufficiency of Zylbertrest's condition is proved.

The equation of this Zylbertrest or Z line is

$$x\cos(T+t)+y\sin(T+t)=\cos T-2\prod_0^{n-1}\sin\tfrac{1}{2}(t_i-t)$$

where $$T=T_0^{n-1}=\sum_0^{n-1}\tfrac{1}{2}(t_i-t-\pi)$$

It follows from the symmetry of the parameters in this equation that it does not matter in which order the vertices P_i are taken in forming the original polygon, i.e. *the Z lines of P w.r.t. the polygons defined by different orderings of the set (P_i) are the same.*

3. In general when the points P_i are not concyclic with P the coordinates of each P_i will require two parameters, say t_i and t_i'. The coordinates of P_{k+1}^r may be derived from the coordinates of P_k^r as before by a cyclic permutation $t_i\to t_{i+1}$, $t_i'\to t_{i+1}'$. The equation of L_k^r, the line joining these points, will have coefficients that are functions of parameters with indices $k, k+1,\ldots,k+r+1$. Hence the permutation swallows its tail when $r=n-2$. If the $(n-2)$th pedal is a line then $L_k^{n-2}=L_0^{n-2}$ i.e. L_k^{n-2} is symmetric in the parameters. In this case L_k^r will be symmetric in the parameters with indices $k, k+1,\ldots,k+r+1$ so that the $(r-2)$th pedal of (P_j), $j=0,\ldots,m-1$, will be a line. In particular, if the $(n-2)$th pedal of P w.r.t. (P_i) is a line the first pedal of P w.r.t. any three P_i will be a line.

The necessity of Zylbertrest's condition now follows from the converse of Simson's theorem, for if the $(n-2)$th pedal is a line the first pedal of P w.r.t. $P_0P_1P_k$ is a line so that P_k lies on the circle PP_0P_1.

4. When $n=3$ the Z line is Simson's line. Its equation

$$x\sin S-y\cos S=\sin(S-t)+2\prod_0^2\sin\tfrac{1}{2}(t_i-t)$$

where $S=\tfrac{1}{2}(t_0+t_1+t_2-t)$ may be put in the form

$$(x-\xi)\sin S-(y-\eta)\cos S=0$$

where
$$2\xi = \cos t + \sum_0^2 \cos t_i$$
$$2\eta = \sin t + \sum_0^2 \sin t_i$$

Thus the Simson line of P w.r.t. $P_0P_1P_2$ passes through the point (ξ, η) say K. Since K is the midpoint of the line joining P to the orthocentre of the triangle $P_0P_1P_2$ it lies on the nine-points circle of this triangle. Furthermore the midpoint of the line OK is the centroid of the four points P, P_0, P_1, P_2, so that the *Simson lines of each vertex of a cyclic quadrilateral w.r.t. the triangle formed by the other three are concurrent, the point of concurrence being the common point of the four nine-point circles of the four triangles determined by the vertices of the quadrilateral.*

4.1. These well-known properties of the Simson line do not generalise easily but the property that the Simson lines of diametrically opposed points are perpendicular does. Thus the slope of the Z line of P w.r.t. (P_i) is $\tan\theta$ where $\theta = T_0^{n-1} + t + \frac{1}{2}\pi$ so that the slope of the Z line of the point with parameter $t+\pi$ is $\tan(\theta - \frac{1}{2}n\pi)$. For even n this is $\tan\theta$, for odd n it is $-\cot\theta$. Hence *the Z lines of diametrically opposed points are perpendicular when n is odd, parallel when n is even.*

4.2. It is useful to remember the distinction between even and odd n when considering the limit as $n \to \infty$. In this case the original polygon becomes the circle itself and the first pedal for example is a cardioid. What happens to the Zylbertrest line? Two ways of controlling the passage to the limit as $n \to \infty$ will be considered. In the first case the points P and P_i are equally spaced round the circle so that $t_i = t + (i+1)2\pi/(n+1)$ and
$$T_0^{n-1} = \frac{1}{2}\sum_0^{n-1} \frac{(i+1)2\pi}{n+1} - \tfrac{1}{2}n\pi = 0$$
so that the Z line of P w.r.t. (P_i) is
$$x\cos t + y\sin t = 1 - 2\prod_0^{n-1} \sin\frac{(i+1)\pi}{n+1}$$

Letting $\beta \to 0$ in the formula
$$\sin(n+1)\beta = 2^n \prod_0^n \sin\left(\beta + \frac{r\pi}{n+1}\right)$$
$$\prod_0^{n-1} \sin\frac{(i+1)\pi}{n+1} = (n+1)/2^n$$
and the Z line is
$$x\cos t + y\sin t = 1 - (n+1)/2^{n-1}$$

As $n \to \infty$ this becomes the tangent at P.

In the second case the points of the polygon are equally spaced round the circle so that $t_i = t_0 + i2\pi/n$ and

$$T_0^{n-1} = \frac{1}{2}\sum_0^{n-1}\left(\frac{i2\pi}{n} + t_0 - t - \pi\right) = \frac{1}{2}n(t_0 - t) - \frac{1}{2}\pi$$

The Z line is now

$$x \sin\left\{\frac{1}{2}n(t_0 - t) + t\right\} - y \cos\left\{\frac{1}{2}n(t_0 - t) + t\right\}$$

$$= \sin\frac{1}{2}n(t_0 - t) - 2\prod_0^{n-1} \sin\left\{\frac{i\pi}{n} + \frac{1}{2}(t_0 - t)\right\}$$

and the limit as $n \to \infty$ is indeterminate unless $t_0 = t$, i.e. unless the polygon is spaced with a vertex at P. If this is so the Z line will be

$$x \sin t - y \cos t = 0$$

and every Z line as well as the limit is the diameter through P. In the first case P is not a vertex of the polygon until the limit is taken, in the second it is a vertex from the start. This distinction separates the points of the circle into two types. If n is even diametrically opposed points are of the same type; if n is odd they are of opposite type. Thus if $n \to \infty$ through even values the limits of the Z lines of diametrically opposed points are parallel; if $n \to \infty$ through odd values the limits are perpendicular.

D. G. TAHTA

2977. Two diophantine equations

1. We show that the equation

$$x_1^2 + x_2^2 + x_3^2 + \ldots + x_n^2 = y^2$$

has an infinity of integral solutions for any n.

We observe first that any odd number $2k+1$ is a difference of two squares $(k+1)^2 - k^2$. But the equation

$$x_1^2 + x_2^2 = y_1^2$$

has an infinity of solutions with y_1 odd; since y_1^2 is odd there exist y_2, x_3 such that $y_1^2 = y_2^2 - x_3^2$ and so

$$x_1^2 + x_2^2 + x_3^2 = y_2^2$$

and y_2 is odd. Hence there exist y_3, x_4 such that $y_2^2 = y_3^2 - x_4^2$,

$$x_1^2 + x_2^2 + x_3^2 + x_4^2 = y_3^2$$

and so on.

2. We determine integral solutions of the equation

$$x_1^4 + x_2^4 + x_3^4 = y_1^4 + y_2^4 + y_3^4 \tag{2.1}$$

Let $a_i = \alpha_i^2 - \beta_i^2$, $b_i = 2\alpha_i\beta_i$, $c_i = \alpha_i^2 + \beta_i^2$, $i = 1, 2$, so that

$$a_i^2 + b_i^2 = c_i^2, \quad i = 1, 2.$$

If in addition

$$a_1 b_1 = a_2 b_2$$

then

$$(\alpha_1^2 - \beta_1^2)\alpha_1\beta_1 = (\alpha_2^2 - \beta_2^2)\alpha_2\beta_2 \tag{2.2}$$

which is satisfied if

$$\alpha_1 + \beta_1 = \alpha_2 + \beta_2, \quad \alpha_1 - \beta_1 = \alpha_2$$

$$\alpha_1\beta_1 = (\alpha_2 - \beta_2)\beta_2$$

whence $2\alpha_1 = 2\alpha_2 + \beta_2$, so that β_2 is even, and $\beta_1 = \frac{1}{2}\beta_2$, and further

$$\beta_2(2\alpha_2 + \beta_2) = 4\alpha_2\beta_2 - 4\beta_2^2$$

so that $\alpha_2 = \frac{5}{2}\beta_2$ and finally $\alpha_1 = 3\beta_2$.

Squaring both sides of the equations

$$a_i^2 + b_i^2 = c_i^2$$

we find

$$c_1^4 - a_1^4 - b_1^4 = 2a_1^2b_1^2 = 2a_2^2b_2^2 = c_2^4 - a_2^4 - b_2^4$$

$$a_1^4 + b_1^4 + c_2^4 = a_2^4 + b_2^4 + c_1^4$$

where (writing $\beta_2 = 2$)

$$a_1 = 35, \quad b_1 = 12, \quad c_1 = 37$$

and

$$a_2 = 21, \quad b_2 = 20, \quad c_2 = 29.$$

11-1-243 *Sitafalmandi* D. RAMESWAR RAO
Secundrabad

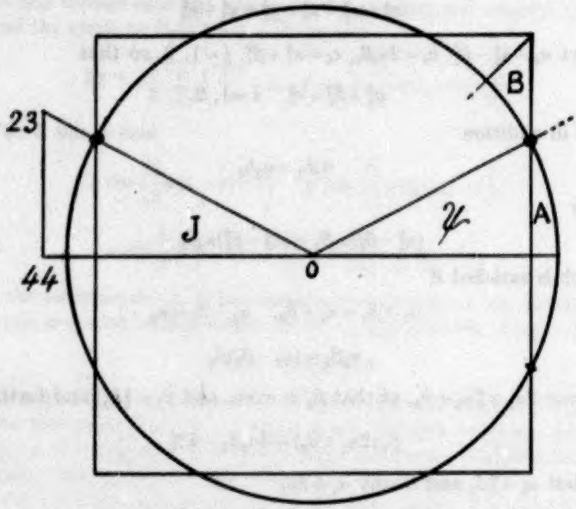
Editorial Note

A two parameter solution of (2.2), leading to a two parameter solution of (2.1) is

$$\alpha_1 = \alpha_2 = h^2 + hk + k^2, \quad \beta_1 = h^2 - k^2, \quad \beta_2 = 2hk + k^2$$

(see R. L. Goodstein, *Gazette* XXIII (1939), p. 266).

2978. Approximate quadrature of the circle



In the above figure the areas of the square and the circle are equal if the areas A and B are equal, and therefore if $\cos^2\psi = \frac{\pi}{4}$, that is

$$\tan\psi = \cdot 5227232\ldots;$$

but $$23/44 = \cdot 5227272\ldots$$

and so we obtain a good approximation by taking $\tan J = 23/44$.

6 Monmouth St., London W.C. 2 LOUIS LOYNES

Editorial Note

The Editor would welcome information on the history of this construction.

2979. Applications of the inequality of the means

I give two examples of applications of the well known inequality between the arithmetical and geometrical means

1. To prove $\lim\limits_{n\to\infty} n^{1/n} = 1$

We have

$$1 < n^{1/2n} = \{n^{n/2}\}^{1/n^2} < \{1 + 1 + \ldots + 1 + \sqrt{n} + \sqrt{n} + \ldots + \sqrt{n}\}/n^2$$

the numerator containing $n^2 - n$ units and $\surd n$ repeated n times, and so

$$1 < n^{1/2n} < 1 - 1/n + 1/\surd n < 1 + 1/\surd n$$

whence

$$1 < n^{1/n} < 1 + 3/\surd n$$

and the result follows.

2. To prove $(1 + 1/n)^n$ is an increasing function of n.

We have

$$\left\{\left(1 + \frac{1}{n}\right)^n\right\}^{1/(n+1)} < \left\{1 + \left(1 + \frac{1}{n}\right) + \left(1 + \frac{1}{n}\right) + \dots + \left(1 + \frac{1}{n}\right)\right\} \Big/ (n+1)$$

$$= \frac{1 + (n+1)}{n+1} = 1 + \frac{1}{n+1}$$

whence

$$\left(1 + \frac{1}{n}\right)^n < \left(1 + \frac{1}{n+1}\right)^{n+1}$$

J. St.-C. L. Sinnadura

2980. Residues of some binomial coefficients

For positive integers a and b, and primes $p > 3$,

$$\binom{ap^b - 1}{p - 1} \equiv 1 \pmod{p^{b+2}}$$

1. When $a = b = 1$, $\binom{p-1}{p-1} = 1$.

When $a > 1$, $b = 1$,

$$\frac{(ap-1)!}{(p-1)!} = (ap-1)(ap-2)\dots\{ap-(p-1)\}.$$

$$= Mp^3 + a^2p^2(p-1)! \sum_{r>s}^{p-1} \frac{1}{rs} - ap(p-1)! \sum_{1}^{p-1} \frac{1}{r} + (p-1)!$$

By a theorem of Hardy and Wright*, p divides $(p-1)! \sum_{r>s}^{p-1} \frac{1}{rs}$,

and by Wolstenholme's theorem†, p^2 divides $(p-1)! \sum_{1}^{p-1} \frac{1}{r}$.

* *Theory of Numbers*, Theorem 113, p.86.
† *Theory of Numbers*, Theorem 115, p.88.

Thus $\dfrac{(ap-1)!}{(p-1)!} = Np^2 + (p-1)!$, so that

$$\binom{ap-1}{p-1} = \frac{Np^3}{(p-1)!} + 1,$$

and since $(p-1)!$ is prime to p, we have proved

$$\binom{ap-1}{p-1} \equiv 1 \pmod{p^3}.$$

2. When $b > 1$, the same argument holds, although it is not necessary to consider the term involving $\sum \frac{1}{rs}$ as this has a factor p^{2b}, and $2b \geqslant b+2$.

$$\left[\text{When } p=3;\ \binom{a3^b-1}{2} = \tfrac{1}{2}(a^2 3^b - 1)(a3^b - 2) = \tfrac{1}{2}(a^2 3^{2b} - a3^{b+1} + 2) \equiv 1 \pmod{3^{b+1}}\right]$$

12 *Grove Lane, Knottingley, Yorks* A. SUTCLIFFE

GLEANINGS FAR AND NEAR

1961. The Snowy Tree Cricket

Their vibratos are based on repetitions of a single syllable, slowly uttered in a monotonous, melancholy tune . . . throughout the night. Only the tempo varies; but it varies so consistently with changes of temperature that it is known as the " temperature cricket ". It is said that if 40 be added to the number of notes per minute divided by 4, the result will approximate the number of degrees Fahrenheit.—Blanche Stillson, *Wings, Insects, Birds and Men*. [Per Mr. W. H. Cozens.]

1962. Kronecker said " God made the integers, all the rest is the work of man ". It might be added " and the Devil made zero! Multiply by zero and all your work vanishes: divide by zero and it becomes nonsense." Did not Mephistopheles say " Ich bin der Geist der stets verneint " (" I am the spirit of negation ") in *Faust*. [Per Roger North.]

1963. In June 1895 she was graduated. Like many another artist she had had her struggles with mathematics; not until the end of her senior year had she succeeded, and then only by special instruction, in removing her deficiency in the first-year course in that discipline.—E. K. Brown, *Willa Cather*, A critical biography (Alfred A. Knopf, New York, 1953). [Per Mr. F. Bowman.]

CLASS ROOM NOTES

75. Reciprocals by iteration

A boy's usual introduction to the important process of iteration is by way of Newton's formula for the solution of $f(x)=0$; $x_{n+1}=x_n-f(x_n)/g$, where g is the gradient of $f(x)$ in the neighbourhood of the zero. (The formula $x_{n+1}=x_n-f(x_n)/f'(x_n)$ is usually more tedious in computation and may even be less rapidly convergent.) There is, however, something to be said for introducing iteration by means of a process which requires less computational skill, and the formula $x_{n+1}=x_n(2-ax_n)$ converging to $x=1/a$ seems admirably suitable. " Division by multiplication " quickly arouses interest.

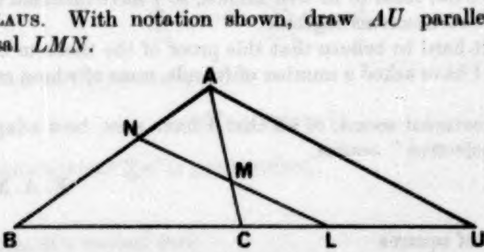
The following is a possible approach. At any stage of the process $1-ax_n$ is to be small; we then want to have $1-ax_{n+1}$ very much smaller. The suggestion $1-ax_{n+1}=(1-ax_n)^2$ seems an obvious choice and leads rapidly to the formula. The convergence rate is thus known; we may expect to double the number of significant figures at each stage. For example, taking $a=3$, $x_1=\cdot 3$ we obtain $x_2=\cdot 33$, $x_3=\cdot 3333$, and so on, x_n having 2^{n-1} digits of the recurring decimal.

Similar considerations can be used to lead to the iterative process for square root. If we want $x_{n+1}-\sqrt{a}$ to be of the order $(x_n-\sqrt{a})^2$, we can write $x_{n+1}-\sqrt{a}=k(x_n-\sqrt{a})^2=k(x_n+a)-2kx_n\sqrt{a}$. The formula will not involve the unknown $\sqrt{a}$ only if $2kx_n=1$, and this leads to $x_{n+1}=\frac{1}{2}(x_n+a/x_n)$.

Sherborne School H. M. Cundy

76. The theorems of Menelaus and Ceva

Menelaus. With notation shown, draw AU parallel to the transversal LMN.



Then, the sense being correct since $ML \parallel AU$,

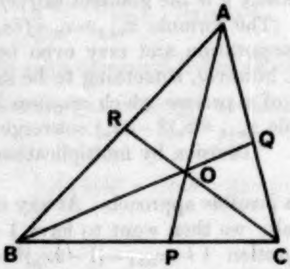
$$\overrightarrow{CM}/\overrightarrow{MA}=\overrightarrow{CL}/\overrightarrow{LU},$$

and, since $NL \parallel AU$,

$$\overrightarrow{AN}/\overrightarrow{NB} = \overrightarrow{UL}/\overrightarrow{LB}$$

Thus, having regard to sense,

$$\frac{BL}{LC}\cdot\frac{CM}{MA}\cdot\frac{AN}{NB} = \frac{BL}{LC}\cdot\frac{CL}{LU}\cdot\frac{UL}{LB} = -1.$$



CEVA. By the theorem of Menelaus, and having regard to sense, $\triangle APC$ gives

$$\frac{CQ}{QA}\cdot\frac{AO}{OP}\cdot\frac{PB}{BC} = -1,$$

and $\triangle APB$ gives

$$\frac{AR}{RB}\cdot\frac{BC}{CP}\cdot\frac{PO}{OA} = -1.$$

Multiply corresponding sides of these two equations:

$$\frac{BP}{PC}\cdot\frac{CQ}{QA}\cdot\frac{AR}{RB} = +1.$$

REMARKS. This proof of the theorem of Menelaus is not new; but it does not seem to be well known, so I have included it here to keep the two theorems together.

I find it hard to believe that this proof of the theorem of Ceva is new, but I have asked a number of friends, none of whom recognised it.

This treatment seems, of all that I have seen, best adapted to a "near-projective" course.

E. A. MAXWELL

77. Runs of squares

Classroom note 66 explains one way in which the 3, 4, 5 triad can be used as a starting point for a lesson in generalisation. Here is another which I have used:

Taking the two statements

$$3^2+4^2=5^2 \quad \text{and} \quad 10^2+11^2+12^2=13^2+14^2$$

a class with a knowledge of quadratic equations can grapple with the problem of finding the " next " one or two. They turn out to be

$$21^2+22^2+23^2+24^2=25^2+26^2+27^2$$

and $$36^2+37^2+38^2+39^2+40^2=41^2+42^2+43^2+44^2$$

In the process of finding these results the class will, with any luck at all, notice:

(i) that any such sequence will lead to a quadratic
(ii) that there is always a set of solutions symmetrical about zero
(iii) that the constant term in their quadratic is always negative
(iv) that positive integral solutions always exist

With these results before them they can then predict the next one or two and test them.

A general approach to this problem, viz the solution of

$$\sum_{0}^{n-1}(a+n)^2=\sum_{n}^{2n-2}(a+n)^2$$

would appear to be suitable for the Sixth although I have not used it myself. It involves $\sum n$ and $\sum n^2$ and leads to the quadratic equation

$$a^2-2(n-1)^2a-(n-1)^2(2n-1)=0.$$

9 Elmoor Avenue, Welwyn, Herts. T. H. Beldon

Editorial Note

A simpler solution is found from the equation

$$\sum_{r=0}^{n}(a-r)^2=\sum_{r=1}^{n}(a+r)^2$$

which leads to the linear equation

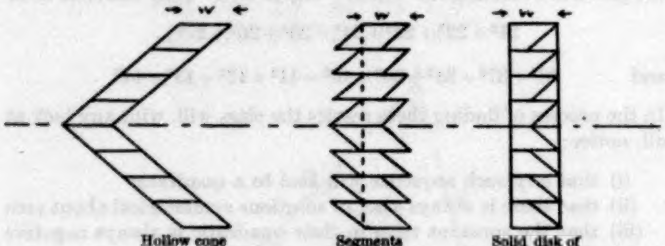
$$a=4\sum n=2n(n+1)$$

and a knowledge of $\sum n^2$ is not involved.

78. Inertia of a conical shell

The moment of inertia of a right hollow cone (or conical shell) of base radius, a, and mass m, about the axis of symmetry, is $\frac{1}{2}ma^2$.

This being the same result which one obtains for the solid cylinder of radius a, mass m, about the same axis, suggests that the cylinder may be obtained from the cone by translations parallel to the axis. The following self evident illustrations show this to be the case.



Rutherford College of Technology D. BOLAM
I. WILKINSON

79. To find the centre of gravity of an orange section of a sphere

Let the semi-circular faces contain an angle 2θ. If an elemental "orange section" has a centre of gravity at a distance x from its straight edge, the whole body is statically equivalent to a uniform arc of radius x subtending 2θ at its centre; and the c.g. of this is $\dfrac{x \sin \theta}{\theta}$ from the centre of the sphere.

In the case of a hemisphere, $\dfrac{x \cdot 1}{\pi/2} = \dfrac{3}{8}a$. Hence the required distance is $\dfrac{3\pi}{16}\dfrac{a \sin \theta}{\theta}$.

As a corollary, letting $\theta \to 0$, the c.g. of a semi-circular lamina with density proportional to the distance from the bounding diameter is $\dfrac{3\pi}{16}a$ from that diameter.

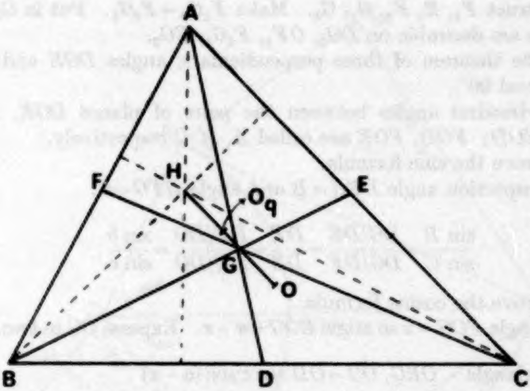
City of London School R. E. GREEN

80. A triangle property

D, E, F are mid-points of the sides of triangle *ABC*.

The triangles *ABC* and *DEF* are homothetic, their homothetic centre being *G*, the centroid, and their homothetic ratio 2 : 1.

If *H* is the orthocentre of *ABC* and *HG* be produced to *O* so that $HG : GO = 2 : 1$, then *O* is the orthocentre of *DEF*.



Therefore DO is perpendicular to EF and therefore to BC

Hence DO is the perpendicular bisector of BC. Similarly EO and FO are the perpendicular bisectors of CA and AB.

Therefore O is the circumcentre of ABC. Thus the orthocentre H, the centroid G, and the circumcentre O of ABC are collinear, and $HG : GO = 2 : 1$.

Again if O_9 is taken on OG produced (i.e. on GH) so that

$$OG : GO_9 = 2 : 1,$$

then O_9 is the circumcentre of DEF.

Hence O_9 is the centre of the Nine-Point circle of ABC, and O, G, O_9 are collinear and $OG : GO_9 = 2 : 1$.

Therefore the four points O, G, O_9, H are collinear. If $GO_9 = x$, $OG = 2x$, $GH = 4x$, $OO_9 = 3x$, $OH = 6x$, and O_9 is the mid-point of OH. Also $OG : GO_9 = 2 : 1$, and $OH : HO_9 = -2 : 1$.

Hence OO_9 is divided harmonically at G and H.

17 *Highfield Villas, Mold, Flinthshire* J. JONES

81. A model for spherical trigonometry

A model to illustrate proofs of

$$(1)\ \frac{\sin a}{\sin A} = \frac{\sin b}{\sin B} = \frac{\sin c}{\sin C} \quad \begin{array}{l}(2)\ \cos c = \cos a \cos b + \sin a \sin b \cos C.\\ (3)\ \cot c \sin a = \cos a \cos B + \sin B \cot C.\end{array}$$

may be made as follows.

The model is intended to be made of drawing paper.

The sequence of construction of the net is as follows.

OD is made about 4 inches long. Next the angles of 65°, 55°. 75°.

Construct F_1, E, F_2, G_1, G_2. Make $F_1G_3 = F_2G_1$. Put in G_3.

Flaps are desirable on DG_3, OF_1, F_2G_1, EG_2.

By the theorem of three perpendiculars, angles DGE and DGF are proved 90°.

The dihedral angles between the pairs of planes DOE, DOF; EOF, EOD; FOD, FOE are called A, B, C respectively.

To prove the sine formula.

By inspection angle $DEG = B$ and angle $DFG = C$.

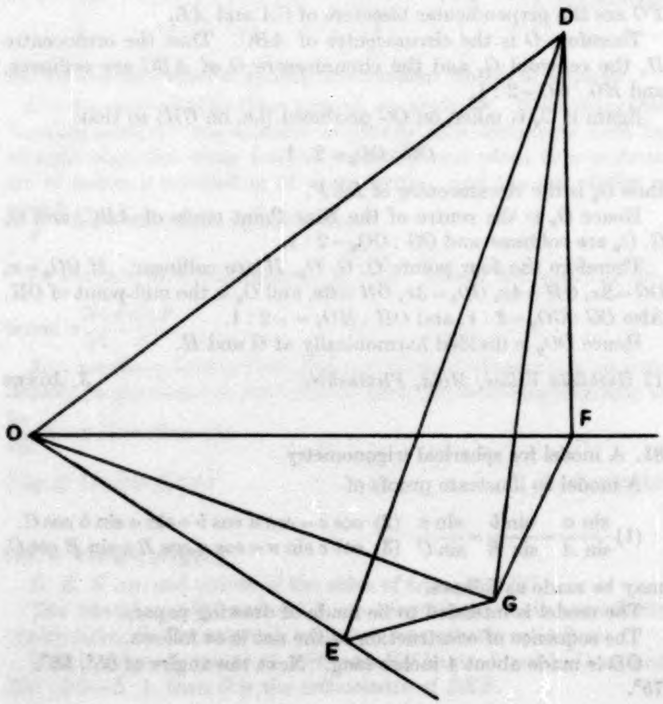
$$\frac{\sin B}{\sin C} = \frac{DG/DE}{DG/DF} = \frac{DF}{DE} = \frac{DF/DO}{DE/DO} = \frac{\sin b}{\sin c}.$$

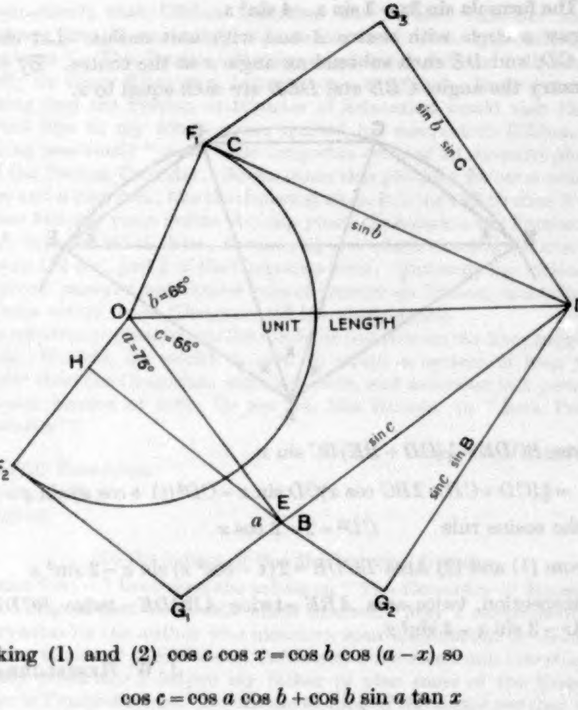
To prove the cosine formula.

Let angle $FOG = x$ so angle $EOG = a - x$. Express OG in two ways.

From triangle OEG, $OG = OD \cos c \sec (a - x)$ (1)

From triangle OFG, $OG = OD \cos b \sec x$ (2)





Linking (1) and (2) $\cos c \cos x = \cos b \cos (a - x)$ so

$$\cos c = \cos a \cos b + \cos b \sin a \tan x \tag{3}$$

It is now seen that we need to prove that $\sin b \cos C = \cos b \tan x$, i.e., to prove

$$\tan b \cos C = \tan x. \tag{4}$$

From the model

$$\tan b \cos C = \frac{DF}{OF} \times \frac{GF}{DF} = \frac{GF}{OF} = \tan x \tag{5}$$

Linking (3) and (5) we have $\cos c = \cos a \cos b + \sin a \sin b \cos C$.

To prove the cotangent formula, using the net.

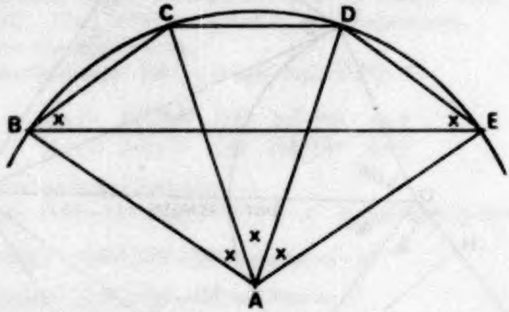
The right hand side is $\cos a \cos B + \sin B \cot C$

$= \cos a \,.\, EG/ED + DG \,.\, FG/DE \,.\, DG = (\cos a \,.\, EG + FG)/DE$
$= EH/DE = \cos c \sin a/\sin c = \cot c \sin a =$ left hand side.

Wyggeston School, Leicester J. W. Hesselgreaves

82. The formula $\sin 3x = 3\sin x - 4\sin^3 x$.

Draw a circle with centre A and with unit radius. Let chords BC, CD, and DE each subtend an angle x at the centre. By circle geometry the angles CBE and DEB are each equal to x.



Area $BCDE = \frac{1}{2}(CD + BE)BC \sin x$

$$= \tfrac{1}{2}(CD + CD + 2BC\cos x)CD \sin x = CD^2((1 + \cos x)\sin x \qquad (1)$$

By the cosine rule $\qquad CD^2 = 2 - 2\cos x \qquad (2)$

From (1) and (2) Area $BCDE = 2(1 - \cos^2 x)\sin x = 2\sin^3 x \qquad (3)$

By inspection, twice area ABE = twice $ABCDE$ − twice $BCDE$ so $\sin 3x = 3\sin x - 4\sin^3 x$.

J. W. HESSELGREAVES

CORRESPONDENCE

To the Editor of the *Mathematical Gazette*

Calendar Reform

DEAR SIR,—I heartily welcome Mr. W. F. Bushell's plea for the reform of our absurd Calendar, with its scars of Augustus's vanity and the ten-month year. In your history books you can read that Charles I was executed in 1649; but a stone in the aisle of St. George's Chapel at Windsor commemorates his burial there in 1648, a fine instance of ὕστερον πρότερον (the cart before the horse). The royal undertaker was clearly still beginning his year in March, probably on March 25th, which was chosen as Annunciation Day because it was the anniversary of the Creation of the World—so I was told by the late M. R. James, O.M., who, if not actually present at the Creation, at any rate began life nearer to it than I did.

Much rummaging in Gibbon's *Decline and Fall* has failed to verify an ancient note (surely not the product of my own disordered

imagination?) that Gibbon describes the *Persian* Calendar as " in accuracy greatly exceeding the Julian, and even approaching the Gregorian ". This Persian Calendar was instituted in the 11th century, chiefly by Omar Khayyam, better known as a poet. Many years ago, hearing that the Persian ex-Minister of Education would visit Eton, I invited him to my school-room, quoted (or misquoted) Gibbon, and (having previously " done " the Gregorian error of 26 seconds) plunged into the Persian Calendar. Seven times this provides for *three* ordinary years and a leap year, like the Julian system, but the eighth time it gives us *four* 365-day years before the leap year. This makes the Persian year $365\frac{8}{33}$ days, or 365 d., 5 hr., 49 min., $5\frac{1}{2}$ sec. which exceeds the true year by only $19\frac{1}{2}$ sec., just $\frac{3}{4}$ of the Gregorian error. Naturally my visitor was delighted, passed a unanimous vote of censure on Gibbon, and talked to the boys about Omar Khayyam till the clock struck.

In addition to cleaning up the inside of our year on the lines suggested by Mr. Bushell, we would do well to adopt a system of leap years simpler than the Gregorian, more accurate, and recurring in a period of 33 years instead of 400. Or are we, like Horace, to " hate Persian apparatus "?

Yours, W. Hope-Jones

Grist Hill Farmhouse,
Shamley Green,
Guildford.

To the Editor of the *Mathematical Gazette*

Dear Sir,— I have read the article on " The Geometry of Megalithic Man " in your last issue with much interest. May I supplement one observation by the author who mentions solar alignments, by means of outlying stones or otherwise, in Caithness, Sutherland and elsewhere.

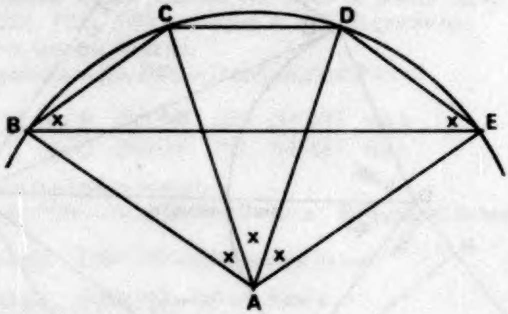
Sixty years ago I helped my father to plan some of the Prescelly circles in Pembrokeshire. Before the coming of the motor car they were little known, but their comparative abundance led my father to write of this area as the " Westminster Abbey " of Prehistoric man, a phrase that was quoted by Dr. Thomas the geologist when, soon after the First World War, he proved that the blue stones at Stonehenge came from that area. Some of these circles had outlying stones pointing to sunrise at the summer solstice, similar to the more famous case at Stonehenge. I also found other solar alignments, not only there, but elsewhere as well.

Sir Norman Lockyer examined these monuments from an astronomical standpoint, and published his book *Stonehenge* in 1906. He claimed that some alignments, by means of outlying stones, were to the rising of well known bright stars. Owing to the procession of the equinoxes, and consequently the movement of these stars in declination, such stellar alignments were no longer accurate, and the age of the monument could be calculated. This has not met with general acceptance, but *solar* alignments were certainly employed in some cases. It is interesting for instance to find that all the remaining megalithic passage graves in the Channel Islands point to sunrise on some day in the year, and other examples could be quoted.

Yours faithfully, W. F. Bushell

82. The formula $\sin 3x = 3 \sin x - 4 \sin^3 x$.

Draw a circle with centre A and with unit radius. Let chords BC, CD, and DE each subtend an angle x at the centre. By circle geometry the angles CBE and DEB are each equal to x.



Area $BCDE = \frac{1}{2}(CD + BE)BC \sin x$

$$= \tfrac{1}{2}(CD + CD + 2BC \cos x)CD \sin x = CD^2((1 + \cos x) \sin x \qquad (1)$$

By the cosine rule $\qquad CD^2 = 2 - 2 \cos x \qquad (2)$

From (1) and (2) Area $BCDE = 2(1 - \cos^2 x) \sin x = 2 \sin^3 x \qquad (3)$

By inspection, twice area ABE = twice $ABCDE$ − twice $BCDE$ so $\sin 3x = 3 \sin x - 4 \sin^3 x$.

J. W. Hesselgreaves

CORRESPONDENCE

To the Editor of the *Mathematical Gazette*

Calendar Reform

Dear Sir,—I heartily welcome Mr. W. F. Bushell's plea for the reform of our absurd Calendar, with its scars of Augustus's vanity and the ten-month year. In your history books you can read that Charles I was executed in 1649; but a stone in the aisle of St. George's Chapel at Windsor commemorates his burial there in 1648, a fine instance of ὕστερον πρότερον (the cart before the horse). The royal undertaker was clearly still beginning his year in March, probably on March 25th, which was chosen as Annunciation Day because it was the anniversary of the Creation of the World—so I was told by the late M. R. James, O.M., who, if not actually present at the Creation, at any rate began life nearer to it than I did.

Much rummaging in Gibbon's *Decline and Fall* has failed to verify an ancient note (surely not the product of my own disordered

imagination?) that Gibbon describes the *Persian* Calendar as "in accuracy greatly exceeding the Julian, and even approaching the Gregorian". This Persian Calendar was instituted in the 11th century, chiefly by Omar Khayyam, better known as a poet. Many years ago, hearing that the Persian ex-Minister of Education would visit Eton, I invited him to my school-room, quoted (or misquoted) Gibbon, and (having previously "done" the Gregorian error of 26 seconds) plunged into the Persian Calendar. Seven times this provides for *three* ordinary years and a leap year, like the Julian system, but the eighth time it gives us *four* 365-day years before the leap year. This makes the Persian year $365\frac{8}{33}$ days, or 365 d., 5 hr., 49 min., $5\frac{1}{2}$ sec. which exceeds the true year by only $19\frac{1}{2}$ sec., just $\frac{3}{4}$ of the Gregorian error. Naturally my visitor was delighted, passed a unanimous vote of censure on Gibbon, and talked to the boys about Omar Khayyam till the clock struck.

In addition to cleaning up the inside of our year on the lines suggested by Mr. Bushell, we would do well to adopt a system of leap years simpler than the Gregorian, more accurate, and recurring in a period of 33 years instead of 400. Or are we, like Horace, to "hate Persian apparatus"?

Yours, W. Hope-Jones

Grist Hill Farmhouse,
Shamley Green,
Guildford.

To the Editor of the *Mathematical Gazette*

Dear Sir,— I have read the article on "The Geometry of Megalithic Man" in your last issue with much interest. May I supplement one observation by the author who mentions solar alignments, by means of outlying stones or otherwise, in Caithness, Sutherland and elsewhere.

Sixty years ago I helped my father to plan some of the Prescelly circles in Pembrokeshire. Before the coming of the motor car they were little known, but their comparative abundance led my father to write of this area as the "Westminster Abbey" of Prehistoric man, a phrase that was quoted by Dr. Thomas the geologist when, soon after the First World War, he proved that the blue stones at Stonehenge came from that area. Some of these circles had outlying stones pointing to sunrise at the summer solstice, similar to the more famous case at Stonehenge. I also found other solar alignments, not only there, but elsewhere as well.

Sir Norman Lockyer examined these monuments from an astronomical standpoint, and published his book *Stonehenge* in 1906. He claimed that some alignments, by means of outlying stones, were to the rising of well known bright stars. Owing to the precession of the equinoxes, and consequently the movement of these stars in declination, such stellar alignments were no longer accurate, and the age of the monument could be calculated. This has not met with general acceptance, but *solar* alignments were certainly employed in some cases. It is interesting for instance to find that all the remaining megalithic passage graves in the Channel Islands point to sunrise on some day in the year, and other examples could be quoted.

Yours faithfully, W. F. Bushell

To the Editor of the *Mathematical Gazette*

DEAR SIR,—Re "Going one better in geometric dissections" by H. Lindgren: In "Modern Puzzles" H. E. Dudeney No. 111 shows how to cut a Maltese cross into seven pieces which will form a square, (solution by A. E. Hill). This is one better than H. Lindgren's eight piece solution.

Re Maestro Puzzles:

The 6×10 rectangle has 2339 solutions as given in number code by the Univ. Manchester electronic computer (C. B. Haselgrove). These results have been translated into drawings by R. A. Fairbairn of Montreal.

The puzzle with the added square in central position has been solved by computer in 65 ways (Dana Scott, Princeton University). I had found 61 of these before seeing the computer results. With the square in positions other than the central one I have so far found and recorded over 7,000 solutions (without of course mirror images). There are strong indications that the final total will be about 10,000.

Yours faithfully, MAURICE J. POVAH

A HISTORIC COLLECTION OF MATHEMATICAL MODELS

Recently, as a result of the sale of some property belonging to the Herschel family, there came to light a chest of drawers packed with mathematical models which were evidently made by Prof. A. S. Herschel about the year 1890. The labels which accompany them indicate that some were exhibited at Munich in 1893 at an Exhibition sponsored by the Deutsche Mathematike Vereinigung; others were shown at the Royal Institution in 1897.

Most of the models are of wood and seem to represent types of crystal structure. They consist of cubes and other polyhedra strung on wire so as to show the changes produced by rotation and shear. Some of the cubes are covered with elaborate chess-board patterns painted in various colours. The meaning of these is not clear, but they may relate to contemporary ideas of molecular structure. There are models entitled "Full and Half repletions of Solid Space", and others whose significance can only be surmised. There is a label, but unfortunately no models, entitled "Plaiting Process of making Crystal Models, devised and exhibited by John Gorham, M.R.C.S., Tonbridge, Kent". Presumably the models were returned to the exhibitor. Other oddities include a Mobius strip made from part of a fashion plate, a linkage of kites and rhombi in a match-box, and a square sheet of paper elaborately folded into a plane octagon with folds ingeniously tucked. If any reader is interested in this collection of models, he can obtain particulars from Miss W. A. Cooke, 6 Cherry Orchard, Stoke Poges, Bucks, to whom they now belong.

WANTED

HOBSON, E. W., *Theory of Spherical and Ellipsoidal Harmonics* (C.U.P. 1931). Please write to the Librarian, Rutherford College of Technology, Newcastle upon Tyne, 1.

REVIEWS

Mathematics and the Physical World. By Morris Kline. Pp. 482. 25s. 1960. (Murray).

This is a companion piece to Kline's *Mathematics in western culture*, in which the author was chiefly concerned to show mathematics as a great creative art. Here his purpose is to exhibit its role as the prime tool in the search for scientific truth and natural knowledge. To him, there is no real distinction between these functions; neither can make a significant advance without the other. The Vatican Aphrodite in the one, the electrons in the other, do not represent opposing cultures, but are both part of man's inheritance which he can most fully appreciate if he is something of a mathematician.

The order in the new book is roughly chronological; in the earlier parts arithmetic, algebra and geometry are used to describe astronomy, particle motion, gravitation, optics, vibrations, as far as these can be described without the calculus. Then, late in the book, the calculus and differential equations are explained with the minimum of technical equipment, and some of their applications are briefly exhibited. I would prefer a less rigid adherence to chronology, so that calculus could be used early in the book to show how its discovery is correlated to the 17th century change of view, the change from a static or steady motion universe to a dynamic, flux-state universe in which rates of change become more important than change. On the other hand, the keen inquirer who wants to know why it is that mathematics is the key to our knowledge of the physical world but has no equipment beyond elementary mathematics (and there are many such people in the world today) will get a surprisingly long way in Kline's hands, and so may be inspired to learn some calculus from this book and pursue its applications elsewhere. Both of Kline's books can be recommended to those seeking mathematics for the layman.

T. A. A. Broadbent

The Search for Order. By C. J. Schneer. Pp. xvii, 398. 21s. 1961. (English Universities Press)

In his foreword, Professor Margenau, the Yale physicist, remarks that few " general education " courses have been successful, since they often merely expose students to a variety of facts, and facts don't integrate. Professor Schneer's book is based on lectures given to students of the " liberal arts ", and is an attempt to describe scientific method rather than scientific facts. The search for natural law, for order and regularity and predictability, is traced from the Greek emphasis on rationality, through the Newtonian insistence on laws proved by phenomena rather than on causes imagined but not verified, through the 19th century revolutions in physics, biology and chemistry, to the 20th century replacement of causality by uncertainty, of determinism by probability. The author is surprisingly successful in avoiding technicalities likely to defeat the non-technical reader, and the book should be

a valuable aid in the important but difficult task of educating the Arts student to a proper appreciation of the impact of science and technology on the human society of today.

T. A. A. B.

The Complete Scientist. Report of the Leverhulme Study Group to the British Association for the Advancement of Science. Pp. xxiii, 162. Cloth, 18s.; paper, 12s. 6d. 1961. (Oxford University Press)

Thanks to a grant from the Leverhulme Trust, the British Association, through a committee headed by Sir Patrick Linstead, has produced this timely report on " the desirability of broadening the education of those intending to become professional scientists and technologists "; mathematicians are included, for convenience, along with the scientists. Every science teacher, in school or university, should study this report carefully; it is penetrating and thorough, though it is a pity that Colleges of Technology do not come into its range.

The committee explains that the terms of reference do not imply that only scientists and technologists need a broader education. In contrast to much heated argument about the two cultures, the report is calm and factual. But its reception will hardly be calm. For instance, a centre-page article in the *Daily Telegraph* on this report asserts dogmatically that " the historian or the scholar of literature who lacks scientific training is less impoverished than the scientist who is ignorant of history or philosophy ". Would it not be better to agree that the ideal is a broad base for all, with specialisation for those who can profit by it, and that the broad base is not necessarily attained by compelling the unwilling scientist to trace the fortunes of Guelf and Ghibelline and the unwilling historian to absorb the doctrine of uniform convergence? An intelligent schoolboy or undergraduate, in a favourable environment, will easily be encouraged to go far beyond the domain of his main studies: but the environment must be favourable. Here I would put more stress than does the report on evidence which shows that " many university candidates do not now get at home the stimulus and support in reading and discussion which they ought to have ". This is a more serious matter than the old bogey, premature specialisation; specialisation is beneficially " early " or harmfully " premature " according as it is in our subject or the other man's.

The report calls for a thorough non-specialist grounding in forms below the sixth, with more time in the sixth for non-specialist work, and supplies a model allocation of time, increasing the allowance for non-specialist work. It explores the possibility of broadening the university course by cutting out dead wood, by postponing very specialised or vocational studies, by lengthening the undergraduate course, and by increasing the provision of post-graduate courses. It refers to the value of extra-curricular courses, to the need for adequate halls of residence. It also refers to the need for more good teachers; it points out that some of its proposals would make greater demands

on the teacher, so that we need not only more teachers but better teachers. And here it fails us. There is a pious suggestion about better pay, which may recruit more teachers but will not recruit more good teachers. What is needed is a sense of vocation, and a recognition, in the community, of the value and importance of that vocation. The *Telegraph* article calls science " a rat-race, with big prizes for the foremost rats ". Does this attitude encourage a young man to become a science teacher, a trainer of rats? Would it not be better to hearken to Dean Gaisford and study Greek, which " fits us for places of emolument not only in this world, but in that which is to come "? The committee is, plainly, convinced that the community needs more good science teachers, but can we be sure that the community shares that conviction? Until it does, we shall not attract the right people in sufficient numbers.

I hope that every science teacher will read this report with care, so that its many admirable suggestions may be grasped and expounded to the community, and so ultimately implemented.

T. A. A. B.

Mathematical Scholarship Problems. By J. C. BURKILL and H. M. CUNDY. Pp. viii, 118. 7s. 6d. (Cambridge University Press)

This excellent book is described as " a selection from recent papers ". It is primarily intended for candidates for University Scholarships in mathematics, either alone or in conjunction with natural sciences. It is hoped that they will also be useful for candidates at advanced and scholarship level in G.C.E. examinations. The book contains 480 problems in pure and applied mathematics, of which at least half are suitable for advanced level candidates, so that the hope should be fulfilled. The questions are grouped, for the most part, according to topics, in sets of five, and the list of contents makes it easy to find questions on a given subject.

Chapter VI consists of forty miscellaneous problems of very different types which will certainly make University Scholarship candidates think extremely hard.

Chapter VII covers 39 pages and consists of hints and solutions, the authors stating in the preface that " the advantages of providing hints and solutions would outweigh any disadvantages ". The hints and solutions in this volume are compiled with such skill that they perhaps form the most valuable part of the work.

The trend in modern mathematics is towards an interest in fundamentals rather than in techniques. This is to some extent reflected by a change in some of the types of question set in University Scholarship papers, and a new selection of problems is certainly due. It is fortunate that it has been so well done.

The printing is of the standard we expect of the Cambridge University Press.

G. A. GARREAU

The Language of Mathematics. By Frank Land. Pp. vi, 264. 21s. Students' edition (flexible binding) 15s. **Exercises.** 3s. 6d. 1961. (John Murray)

Dr. Land's expressed intention is " to give some idea of the intellectual vistas which mathematics opens up.... (The book's) two main underlying ideas are, first, that mathematics is not merely an indispensable tool to the scientist and research worker, but a pursuit which is intensely rewarding in itself, and second, that everyday topics are seen in a new light when considered in mathematical terms ". The readers for whom the book was written are students training to be teachers, teachers and sixth-formers not specialising in mathematics.

The first section of the book begins with a brief historical sketch of ways of counting and recording numbers. Chapters on systems of units and simple calculations separate this from a treatment of directed numbers, fractions and irrational numbers, and the arithmetical part concludes with a chapter on the calendar and a surprisingly lengthy exposition of calculations involving simple and compound units. Sections which are mainly algebraic and geometrical follow. A brief look at the language of set-theory leads to the idea of a functional relationship and its graphical representation. Particular attention is given to the parabola and to the logarithmic and exponential curves and their applications to various practical situations. Geometrical topics touched upon include simple topological properties, paper-folding, rigidity, areas, similarity, orbits and the golden section. The book ends with a chapter on statistics.

This survey will indicate that the content is largely conventional and familiar. (Although sets and topology are referred to they are not dealt with in any detail and their relevance to the rest of the subject matter is not explored.) Where the book is unusual is in the number and range of its examples of the applications of mathematics to the world around. This is an excellent feature. Many sources must have been searched to provide these and they should provoke readers into practising a similar alertness and diligence in looking for the uses of mathematics.

The book is exceptionally well illustrated, achieving a wide range of effect with the use of two-colour drawings. A minor criticism is that the visual representation of place-value on pages 49 and 138 verges on the dishonest. On p. 202 the text refers to a " rectangular solid " where the diagram shows a cube. (This paragraph, though, has such doubtful validity that the confusion is understandable.)

The language of the text is very clear and readable, although oddly inconsistent in level. At times it sounds like a teacher patiently explaining to a small child and at others it appears to be addressed to adults having a fair familiarity with mathematical terms. These variations perhaps reflect some uncertainty about the kind of reader for whom the author is writing.

It is inevitable that with so many topics treated in one book there should be some differences in the skill with which they are presented, but most of the sections will provide the reader with illuminating examples and stimulating discussion. Few teachers will fail to be

impressed by, for example, the ingenuity of development and wealth of illustration in the chapter " Logs, Pianos and Spirals ". There are many wise remarks throughout the book from which teachers and pupils will gain an additional insight into familiar mathematics. For this reason it deserves a place in the school library and probably in the teacher's own library as well. If it needs any extra commendation this will lie in the excellence of the design which sets a completely new standard for book production in this field—moreover, at an incredibly low price.

Unfortunately this review cannot end here. The publishers have made the grandiose claim that this book " offers a ' break-through ' into a new and unfamiliar world of ideas " and that it is an important contribution to the problem of " numeracy ". Such statements cannot be ignored because they seriously mislead even when allowance has been made for professional exuberance. They imply that the non-specialist will be helped by Dr Land's book towards an understanding of the nature of mathematical thinking. Judged by this high standard the book has major flaws.

Two chapters—on systems of units and the calendar—are not concerned, except in a trivial sense, with mathematics at all, and some other items of historical fact and social usage are brought in for their curiosity value only. More important, there is a strong emphasis throughout the book on calculation without a sufficient stress on argument and justification. Whereas the procedures of long division and manipulation of fractions, for example, are treated at length, properties of the parabola and ellipse are quoted without support. In various places proofs are deferred, discussions adjourned and formulae simply appear. This lack of balance can only encourage the view that mathematics is (i) boring and (ii) mysterious.

Even more serious is the lack of an overall view of mathematics which could—explicitly or implicitly—show the unity of the subject underpinning its rich diversity. An atomistic approach in a book aimed at mature students turns the language of mathematics into a string of nonsense syllables. Behind the applications, the manipulative techniques and the results of mathematical thinking, lies mathematical thought itself. If this is not exposed, stripped of its mystery and displayed so that it is seen as a natural, human and inevitable activity, the heart of the matter has not been reached.

This book will be widely read and enjoyed. It is not the book that we are all waiting for, but until that one arrives we are grateful for this.

D. H. Wheeler

The World of Mathematics. Edited by J. R. Newman. Vol. I, pp. xviii, 724; Vol. II, pp. vii, 726–1414; Vol. III, pp. vii, 1416–2021; Vol. IV, pp. vii, 2024–2525. 1960. 7 guineas the set. (George Allen and Unwin Ltd., London.)

Subtitled " A Small Library of the Literature of Mathematics ", this anthology of 133 pieces from over 100 authors, was collected by

the editor over 15 years. It first appeared in the U.S.A. in 1956, and is in the best tradition of the Scientific American, on whose editorial board Mr. Newman sits: that is to say it is intended for the seriously interested layman, is eminently suitable for school children, and at the same time covers so much ground that even the more erudite must find something new. The wonder of this popular survey is that comparatively few of the extracts are from popular works: Mr. Newman has searched widely to find original writings of the right standard. A welcome feature is the inclusion of long extracts—the first 170 of these well but closely printed pages are devoted to two books: P. E. B. Jourdain's *The Nature of Mathematics*, and H. W. Turnbull's *The Great Mathematicians*. Most of the pieces have a page or two of introduction from the editor, which are well informed, compact, and well referenced.

The work is divided into sections, and after general and historical ones, there are those dealing with the relation of mathematics to the sciences—physical, metaphysical, logical, biological, social and statistical. Pure mathematics is then dealt with somewhat scantily, with extracts on a few specific topics, followed by a number of miscellaneous sections, several containing only one item, which would have been better put together as frankly miscellaneous. There are then sections on mathematics and some of the arts, each dealt with in a different way. The section on music consists of long extracts from Jean's *Science and Music*, which deals more with accoustics than with music. There is nothing on the underlying similarities of music and mathematics: has this often discussed subject a literature of its own since Pythagoras?—we cannot blame the anthologer for omitting what does not exist. The graphic arts are better treated, thanks mainly to the two lectures on symmetry by Herman Weyl. Although these are in the section on the Mathematics of Space and Motion, they deal with artistic as much as with natural symmetry. Literature is represented by five stories or extracts with mathematical point. There is no mention of the statistical study of language and style. An alarming transatlantic note is struck in the title, but not the contents, of the penultimate section, " Mathematics as a Culture Clue ". Finally, there is Amusements, Puzzles, Fancies, a miscellany of 10 pieces (two of them wasted, for me, on Stephen Leacock), that is a little disappointing. There is an index of 65 pages.

In his brief preface, Mr. Newman says " an anthology is a work of prejudice ": his is certainly in favour of application. In his note of introduction to extracts from Hardy's Apology, he quotes Soddy's remark " from such cloistral clowning the world sickens ", and later he dismisses Hardy's claim to have done nothing useful as nonsense. More of the anthology is *about* mathematics and its application, less of it *is* mathematics.

The set is well produced and the many illustrations come out well, though the half-tone would have been better on glossier paper. It is a bold attempt at popularisation, not uniformly successful, but deserving to be seen in school libraries, and for its size, not expensive.

ALAN SUTCLIFFE

Scholarship Mathematics, Volume II: Geometry. By A. T. STARR. Pp. viii + 251. 21s. 1961. Pitman.

" The second volume of *Scholarship Mathematics* covers the work in Euclidean, Analytical and Projective Geometry required for the scholarship examinations of the Universities of Oxford and Cambridge." A book with this aim, collecting also a large number of examples from recent papers, seems bound to attract readers. But it ought never to have been written.

Whether any book on such a basis is educationally desirable, I am in any case doubtful; I am quite sure that the present attempt is too inaccurate to stand scrutiny. This is a serious statement, and must be justified at some length, for the author has taken much trouble in the preparation. Let him first speak for himself:
(p. 82). $S + \lambda l = 0$ has the asymptotes of $S = 0$. For the asymptotes are given by equating the second degree terms of S [a general conic] to zero.
(p. 54). When S_1P is very large, ON^2/a^2 and NP^2/b^2 are very large, so they must be nearly equal since their difference is unity. It follows that $NP/ON = b/a = \pm\sqrt{(e^2 - 1)}$. The lines through the origin with slope $\pm\sqrt{(e^2 - 1)}$ thus meet the curves at infinity, i.e. they are the *asymptotes*.
(p. 133). Since the equation of the line at infinity is $z = 0$, i.e. constant $= 0$, the asymptotes have as equation $S + \lambda L\infty^2 = 0$ or $S + \lambda = 0$.
(p. 140). The line $lx + my + n = 0$ meets the curve [given parametrically] where

$$t^2(a_1l + a_2m + a_3n) + 2t(b_1l + b_2m + b_3n) + (c_1l + c_2m + c_3n) = 0.$$

This gives 2 independent values of t [this does not mean equal values: that comes later] provided the coefficients are independent, i.e.

$$|a_1b_2c_3| \neq 0.$$

[What *does* " independent values " mean in this context?]
(p. 165). Therefore the conditions of conjugacy are:

$$bcv'w' + cavu' + abu'w = 0$$

$$bcuv' + cau'w' + abv'w = 0$$

$$bcuw' + cavw' + abu'v' = 0.$$

These equations give $u' = v' = w' = 0$.

[Like that. The six constants u, v, w, u', v', w' are, at this point, at disposal. A solution is $u = \beta - \gamma$, $u' = \sqrt{\{(\gamma - \alpha)(\alpha - \beta)\}}$ etc., where $\alpha = 1/a$ etc.]
(p. 193). Now the transformation of (3.2)

$$[x' = a_{11}x + a_{12}y + a_{13}z, \text{ etc.}]$$

is determined by eight ratios of the coefficients a_{11}, and thus has eight degrees of freedom. In consequence any four points can be projected into four given points.

(p. 129). The centre is at infinity if $C=0$, i.e. $ab-h^2=0$. The curve is then a parabola.
(p. 72). We say that $z=0$ is the line at infinity, since it is a linear equation and all the points have infinite x/z and/or y/z.
(p. 161). A parabola is characterised by having the line at infinity as a tangent so that we take two points on it at infinity. It follows that if A, B, C, D are at finite positions on the parabola, we may complete a hexagon by taking two points, E and F say, at infinity.
(p. 244). When two of the eleven points are taken as I and J, the eleven-point conic reduces to the nine-point circle.

[And that is all; the relevant two points are not shown in the "quadrilateral" diagram and there is no indication of how to move from quadrilateral to triangle.]
(p. 196). Thus concurrence, collinearity, tangency, and order and class of curves are invariant [for conical projection]. The invariant is cross-ratio. [What about the projection of a twisted cubic into a conic?]

I imagine that anyone teaching the subject will agree that these quotations give either actual errors or points of view (for example, of "infinity") which are not nowadays regarded as desirable. Other criticisms may be made briefly, though here the question of personal preference becomes more significant.

There are several solutions which, though accurate, seem to me to be pedestrian and lacking in "Scholarship" quality. For example, the problem on pp. 213–4, here solved in a whole page of algebra, can be done in two or three lines by much more significant projective argument —and no Scholar should test the collinearity of $(1+\lambda, 1, 1)$, $(0, 1, 0)$, $(1+\lambda, 1+\lambda, 1)$ by a determinant. Other solutions are incomplete. For example (p. 41), it is not immediately obvious without some sort of reference that "the perpendiculars to the faces through the incentres" of the triangular faces of a (suitable) tetrahedron meet in a point, nor that a sphere with centre at that point will pass through all the circles. Converses, too, are often confused. For example, p. 211: "If Q_1 is $p_1(x_1, y_1, z_1)$ and Q_2 is $p_2(x_2, y_2, z_2)$, then any point P on the line is $p_1+\theta p_2$. This follows since $(p_1+\theta p_2)-(p_1)-\theta(p_2)=0$."
[This only proves that such a point *is* on the line; there may be others.]

Finally, and this, of course, is personal opinion only, I find the treatment of complex geometry and of "infinite" elements very confusing. The author seems to move between real, complex, finite and infinite with a freedom that is quite bewildering. Surely geometry (of this kind) can be *either* real *or* complex, but never both at once; and I believe that much confusion occurs later if this fact is not made clear as soon as possible.

There is no pleasure in writing a review of this kind. The author, his printers and his publisher have undertaken a great deal of hard work, and the author is obviously skilled in the craft of authorship. The fact remains, though, and I do not see how to avoid stating it, that the subject matter is completely unsatisfactory and unsuitable for anyone intending to prepare for the examinations to which the title refers.

E. A. MAXWELL

Integral Calculus. By W. L. FERRAR. Pp. 276. 35s. 1958. (Clarendon Press, Oxford.)

This is a companion volume to the author's *Differential Calculus* (Oxford, 1956), and is aimed at first and second year undergraduates in both mathematics and science. It consists of three parts, on the indefinite integral, the definite integral, and double and curvilinear integrals.

The first part of the book, consisting of three chapters, contains a very thorough treatment of the problems of indefinite integration. After an introductory chapter, Chapter II gives the three standard devices normally used in indefinite integration, namely substitution, integration by parts, and reduction formulae, and adds a fourth which deserves to be better known, namely differentiation under the integral sign. Chapter III deals with systematic integration of rational functions and of functions which can be reduced to rational form by appropriate substitutions.

The second and third parts contain the more analytical material. The second part begins (Chapter IV) with an intuitive approach to the problem of definite integration, and then passes to the analytical definition of the Riemann integral by means of the upper and lower integrals of Darboux. The standard properties of the Riemann integral are then obtained, and (Chapter V) the methods of evaluating definite integrals are investigated. Chapter VI deals with the convergence of infinite and improper integrals, and Chapter VII contains an account of the problem of differentiation under the sign of integration in a definite integral. This part concludes (Chapter VIII) with a short introduction to the Riemann-Stieltjes integral.

Part III, consisting of six chapters, contains the standard material on double and curvilinear integrals—double integrals, reduction of double integrals to repeated integrals, the Gamma and Beta functions, change of variable in a double integral, surface integrals, curvilinear integrals, and the theorems of Green and Stokes.

The aim of the book appears to be to give the student a mastery of the computational techniques of integration, whilst at the same time stressing the conceptual aspects of the subject. The author carefully limits himself to a treatment which does not take him too far into analysis, but within these limits his approach is completely rigorous. In particular, the treatment of the material in Part III attains a much higher standard of rigour than is usual in books at this level. The style is leisurely and lucid, though it is possible that some readers may find the extreme thoroughness of the book rather intimidating.

It is easy to disagree with the limits which an author sets himself, but in several places in this book the reviewer felt that a more general treatment would have been no more difficult, and a good deal more natural, than the limited discussion given. This is particularly the case in the sections on lengths of curves, curvilinear integrals, and Green's theorem. The reviewer also found it somewhat disconcerting to find essentially different treatments given for the Riemann integral in one dimension, the Riemann-Stieltjes integral, and the Riemann double integral. It

would seem to the reviewer much more satisfactory to show that all these integrals are particular instances of the same theory, than to display the various alternative treatments of the Riemann theory which are possible.

The printing of the book maintains the high standard of the Clarendon Press. The reviewer hopes, however, that the author's use of a formula reference number to represent the relevant expression in subsequent formulae (e.g. the inequality

$$|(4)| \leqslant \int_a^b \frac{\epsilon}{b-a}\, dx$$

on p. 145, where " (4) " is the reference number to the left-hand side of a previous equation) is an innovation which future authors will not copy.

T. M. FLETT

Mathématiques et Mathématiciens. By PIERRE DEDRON and JEAN ITARD. Pp. 433. 1959. n.p. (Magnard, Paris)

This paper-back provides a lively and stimulating survey of mathematics from earliest times to Lagrange, Laplace and Monge, followed by separate discussions of developments during the same period in particular fields of elementary mathematics. The authors are masters of their subject, but they carry their scholarship lightly, and the almost undisciplined scattering of illustrations enhances the atmosphere of excitement. This is probably the best informal introduction to the history of mathematics available, and it is to be hoped that it will soon appear in English.

M. A. HOSKIN

Éléments d'Histoire des Mathématiques. By NICOLAS BOURBAKI. Pp. 276. 18 N.F. 1960. (Hermann, Paris)

This work, described by the publishers as by " le plus grand mathematicien contemporain ", is essentially collected from the historical notes to the volumes of *Éléments de Mathématique*. The treatment is technical and the subject matter limited, but it will be convenient to have the material assembled in this form.

M. A. HOSKIN

Makers of Mathematics. By ALFRED HOOPER. Pp. ix + 402. 12s. 6d. This edition, 1961. (Faber)

This credulous work is designed as a popular introduction to the history of mathematics down to the eighteenth century. Lacking all historical sense and based on an antiquated collection of secondary sources, it has now been republished in an all-too-well-printed paperback edition.

M. A. HOSKIN

Dictionnaire Historique de la Terminologie Géomètrique des Grecs. By CHARLES MUGLER. Vol. 1. Introduction, *A–K*. Pp. 272. 1958. Vol. 2. *Λ–Ω*. Pp. 184. 1959. The set: 220 N.F. (Gauthier-Villars and C. Klincksieck)

This impressive work of scholarship gives for each term used in Greek mathematics the equivalent in Latin, French, German and English, together with a detailed analysis of its use in the ancient texts. There are few fields of the history of science that can as yet afford the luxury of such detailed scholarship, and few historians who can afford to have so expensive a work on their shelves.

M. A. HOSKIN

Introduction to Probability and Statistics. By HENRY L. ALDER and EDWARD B. ROESSLER. Pp. xi + 252. 20s. 1960. (W. H. Freeman and Co. Ltd.)

Probability and Statistics is a mathematics text-book designed for undergraduate students of the natural and social sciences: its study requires little mathematical knowledge other than algebra and the authors suggest that it might be used in high schools as well as universities and colleges.

It provides an interesting account of statistical methods including frequency distributions, sampling theory, regression and correlation. Three chapters are devoted to probability, the binomial and normal distributions. The discussion of significance tests includes the use of student's-*t* and the *chi*-squared distributions as well as a simple non-parametric test. There are also chapters on graphical methods, index numbers and time series. The exercises, which are numerous, are taken from a variety of sciences and numerical answers are given for the odd-numbered exercises.

This text-book has one serious fault. It tends to oversimplify the subject by reducing it to a series of definitions and theorems. Also some of these definitions are only true within a limited context and the reader is not given any warning concerning their limitations. Nevertheless teachers of elementary statistics will find this book very useful. It is well written and the typography is attractive.

FREDA CONWAY

Introduction to Modern Algebra. By J. L. KELLY. Pp. 338. 21s. 1960. **Student Manual.** By ROY DUBISCH. Pp. 63. 7s. 6d. 1960. (D. Van Nostrand, London and New York)

Starting with an axiomatic development of the real number system, which in spite of many digressions on groups, and the algebra of sets, appears to be lacking in motivation, the natural numbers are obtained as a special subclass of the reals. The existence of $\sqrt{2}$ is proved by postulating the existence of the least upper bound of a bounded set, but the irrationality of $\sqrt{2}$ is left to be proved as an example. Next comes a study of ordered pairs (and triples) and the definition that a

set of ordered pairs f is a function if whenever (x, y), (x, z) belong to f then $y = z$. Vectors are applied to plane geometry, and very briefly to three-dimensional geometry. Complex numbers (as two-dimensional vectors) are studied next and also hypercomplex numbers. Independence and orthogonality of sets of 3-vectors follows, with applications to planes, and finally comes a chapter on matrix algebra, in two and three dimensions.

The book, by the author of a well-known volume on Topology, is intended for a first year course in a liberal arts college program, or for a course for high school students in an accelerated program. Presumably the corresponding level in this country is the sixth form in a grammar school. Certainly most mathematics specialists in sixth forms would find the course very easy going, because in spite of the modern sound of many of the topics, only trivial applications of the ideas occur. But it might be used for a course in a Training College, where the emphasis is more on ideas than techniques.

The student manual for the study of Kelly's Algebra, written by Roy Dubisch, is very helpful as a commentary on the text, and supplies hints for the solution of problems. R. L. Goodstein

An Introduction to the Theory of Numbers. By I. Niven and H. S. Zuckerman. Pp. 250. 50s. 1960. (Wiley, London and New York)

The authors' aim to present a reasonably complete introduction to the theory of numbers in a simple volume has been successfully accomplished, the geometry of numbers alone being conspicuously missing. In addition to the familiar topics of an introductory text—congruences, indices, representation by sums of squares (excluding the difficult three squares theorem), some arithmetical functions, continued fractions and Pell's equation—there are accounts of Skolem's solution of the equation $ax^2 + by^2 + cz^2 = 0$, unique factorisation in quadratic fields, Erdös' proof of the existence of a prime in every interval $(n, 2n)$, and Dyson's proof of Mann's theorem. A valuable feature of the book is the very good collection of examples. R. L. Goodstein

Raumbild-Lehrbuch der Darstellenden Geometrie. By Ernst Schörner. Pp. 153. DM 16. 1960. (R. Oldenbourg, Munich)

In addition to the familiar constructions for plane sections of solids this book contains 60 stereoscopic drawings illustrating projections and intersections, for viewing with the red and blue filter spectacles provided. These stereoscopic pictures are magnificent and far surpass any previous production in realism and perfection of detail. They are made to be viewed flat on the table with the viewer sitting upright. Amongst many remarkable models illustrated stereoscopically are intersecting cylinders, spheres inscribed in cones and touching planes at the foci of the plane intersections, and intersecting cones. The model of Desargues' theorem on perspective triangles is particularly instructive. The book is remarkable value for 16 marks, and both author and publisher are to be warmly congratulated on its production.

R. L. Goodstein

Boolean Algebra and its Applications. By J. E. Whitesitt. Pp. 182. 51s. 1961. (Addison-Wesley, Reading, Mass.)

This is an attractively presented introductory account of the elements of Boolean algebra with applications to electrical circuit theory and probability. Starting with the algebra of sets constructed on an intuitive basis we pass to an axiomatisation of Boolean algebra (using the Huntington postulates) and a brief study of normal forms and functional completeness; this is followed by a chapter on sentence logic which is somewhat marred by a failure to distinguish between object and meta-variables in the discussion of rules of inference.

The chapters on electrical circuits are very good and discuss not only switching circuits, but also relays and sequential relay circuits, including the design of circuits for the addition and multiplication of (three digit) numbers. The book concludes with a brief chapter on probability in a finite sample space.

There is a good collection of examples with solutions to many of these at the end of the book. An outstandingly interesting application of the two-way switch circuit is the example in Chapter 3 of the captured logician who is promised his freedom if he can guess which of two boxes contains the key to his room; he is allowed to ask a single question and will not necessarily be answered truthfully. What question wins him his freedom?

R. L. Goodstein

Analytical Quadrics. By Barry Spain. Pp. ix, 135. 1960. 30s. (Pergamon Press)

This book is similar to the author's *Analytical Conics*: the aim is to present the essential features of the subject without going into great detail. Although the title refers only to quadrics, the book is self-contained, and suitable for beginners in three-dimensional geometry.

I have only a few minor criticisms. On page 15, the author says that the equation $\sigma u_1 + \tau u_2 = 0$, where u_1 and u_2 are linear functions in x, y, z, is satisfied by the solution of the equations $u_1 = u_2 = 0$; one cannot talk of *the* solution of two equations in three unknowns. On page 22 A and B are defined as conjugate points for a sphere if the points where AB meets the sphere divide AB harmonically. What happens if AB does not meet the sphere? On page 31, the condition that the equation $ax^2 + by^2 + cz^2 + 2fyz + 2gzx + 2hxy = 0$ represents a circular cone is expressed by saying that the rank of the matrix

$$\begin{vmatrix} a-\lambda & b & g \\ b & b-\lambda & f \\ g & f & c-\lambda \end{vmatrix} \text{ is one.}$$

In spite of a reference to the appendix, this is not clear: what is λ supposed to be?

As an introduction to three-dimensional geometry the book can confidently be recommended. It is carefully written, and there are plenty of examples, with hints for their solution.

E. J. F. Primrose

Cours d'Analyse de l'Ecole Polytechnique, Tome II. By J. Favard. Pp. 578. 80 NF. 1960. (Gauthier-Villars, Paris)

This second volume of the course covers functions of a real variable and functions of a complex variable. The topological ideas which were explained in the first volume are used extensively here.

E. J. F. Primrose

A Primer of Real Functions. By R. P. Boas, Jr. Pp. xi, 189. 32s. 1960. Carus Mathematical Monographs, Number 13. (American Mathematical Society, New York.)

There is a tendency for the modern mathematician to sit in his ivory tower and to concern himself only with the broad sweep of the view which lies within his gaze—he is rather like the landscape gardeners of the eighteenth century who considered anything less than a whole field as unworthy of their attention. It is good, therefore, to be reminded of the interest and the beauty which can be found in the more intensive cultivation of relatively small domains.

In the book under review, we are given some of the results achieved by such an intensive cultivation, in that part of the field of " real variables " which deals particularly with the properties of continuity and differentiability. The book is not a systematic treatise, but is more in the nature of a course of informal lectures; the author has set out to tell readers who have little previous knowledge of the subject some of the results which he has himself found particularly interesting, and he has limited himself to the material that seemed essential for the results he had in mind, together with " as much related material as seemed interesting and not too complicated ". His aim has been, too, " to preserve some of the sense of wonder that was associated with the subject in its early days but has now largely been lost ".

In this last aim Professor Boas has been strikingly successful, and his book presents a fascinating and very readable account of many of the interesting and beautiful results which lie in his chosen field. The sense of wonder is admirably preserved, and the book could well serve to revive the interest in analysis of those students to whom " analysis " at present too often means the proof, in successive annual stages of increasing rigour, of results learnt in outline at school.

The book is divided into two chapters. The first is concerned with the theory of sets of points, in particular with metric spaces, open and closed sets, dense and nowhere dense sets, compactness, convergence and completeness, sets of the first and second category, and sets of measure zero. There are immediate applications of these concepts to functions of a real variable, to give point to the definitions, and there are enough exercises to test the reader's grasp of the material.

The second chapter forms the main part of the book. It begins with a good introduction to the concept of a function which explains clearly the modern usage concerning the notation for functions and their values. Then follows a section on the definition and properties of continuous functions which includes a number of less well-known but very striking

results, for example, the horizontal chord theorem, that if f is continous in the closed interval [0, 1], and $f(0) = f(1)$ (so that the graph of f has a horizontal chord of length 1), then the graph of f has horizontal chords of lengths $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \ldots$, but not necessarily a horizontal chord of any given length that is not the reciprocal of an integer. This discussion of continuous functions is followed by an account of uniform convergence, with various applications to such diverse topics as space-filling curves, and derivatives of infinite order. Other topics dealt with in this part of the book are the theorem of Baire that a pointwise limit of continuous functions is continuous at the points of an everywhere dense set, uniform continuity and the approximation to continuous functions by step-functions and polynomials, the moment theorem for continuous functions, and continuous and discontinuous solutions of the equation $f(x) + f(y) = f(x+y)$.

The remainder of this second chapter is concerned with differentiation and the properties of the derivative and the Dini derivates of a function. The principal result here is the theorem that a monotonic function is differentiable almost everywhere, but there are many other interesting results, and the whole discussion is illuminated by examples of good and bad behaviour. Much of the material that is presented in this chapter is taken from recent papers, and relatively little of it has been accessible in a collected form except in " more forbidding systematic treatises ".

A novel feature of a book at this level is that it includes solutions to the exercises. There is also a good index, and the printing of the book maintains the high standards of the Carus Monographs.

The reviewer's criticisms of the book are very minor ones. In the chapter on sets of points, there are one or two places where a more strictly logical arrangement of the material might have been more helpful to the reader. There is also (pp. 46–7) a rather unhappy definition of an infinite series as the sequence of its partial sums, a definition which does not seem to agree with some of the author's subsequent remarks on the subject. The prospective reader should also be warned that the text of the exercises is sometimes intended to be read as an integral part of the text of the book; the reviewer found this a little disconcerting in several places, particularly on pp. 33 and 37. All these, however, are very small criticisms of an excellent book.

T. M. Flett

Cartesian Tensors. An Introduction. By G. Temple. Pp. 92. 12s. 6d. 1960. Monographs on physical subjects. (Methuen)

Those to whom tensors meant alarming symbols in bizarre spaces were enlightened, 30 years ago, by Sir Harold Jeffreys' exposition of the simplicity and value of tensor analysis in Euclidean three-space. Professor Temple underlines and extends this lesson in his short monograph, which should be easily comprehended by any Honours student.

Among the book's many merits, two in particular deserve notice. Temple follows Bourbaki in recognising a tensor as an invariant multi-

linear function of direction. This makes it easy to answer the novice's question "What *is* a tensor?" The more sophisticated student may understand that how a tensor behaves is more important than what a tensor is, but the definition of a tensor as a vague something which obeys a not very luminous law of transformation must have been a stumbling block to many a beginner. The concept of a multilinear function of direction is easy enough to grasp, particularly when the general notion is prefaced by concrete instances, and the transformation law becomes almost self-evident for tensors of the second rank and is easily generalised to tensors of higher rank. Secondly, there is a short and valuable chapter on spinors. These somewhat subtle entities can be approached either by considering the eigenvectors of a rotation, of which two are complex, or by way of the Pauli spin matrices, the unit rotors for the axes, which exemplify a Clifford algebra. Both methods are slightly devious, and the exposition here is terse, though clearer than that to be found in some other accounts. The reader might be well advised to brush up his knowledge of the Cayley-Klein rotation parameters before reading this chapter; he would then be more likely to appreciate the motivation.

The final chapter, on tensors in orthogonal curvilinear coordinates, is strictly outside the declared purpose of the book, but is a welcome compact discussion of component formulae in these systems.

Of several recent accounts of Cartesian tensors, this is quite the clearest and most elegant. T. A. A. BROADBENT

Annual Review in Automatic Programming, Vol. 1. Edited by RICHARD GOODMAN. Pp. 300. 63s. 1960. (Pergamon Press)

A three-day Conference on the Automatic Programming of Digital Computers was held at Brighton Technical College in April 1959, and the contributions, modified slightly so as to incorporate points made during discussion, have been brought together as the first volume of an annual series. The publication of this book reflects the current change of emphasis in digital computer practice towards the use of *problem-oriented languages*: the computer is addressed, not in its own internal language, but in a symbolic language closely similar to that in which the user habitually formulates problems of a particular kind. Thus there are matrix languages, suitable for the specification of sequences of inversions and transpositions; commercial languages, suitable for the specification of clerical procedures; algebraic languages, statistical languages, and so on. A computer can be programmed so as to respond in a variety of ways to these forms of input, perhaps (e.g., as an *Assembler* or as a *Compiler*) replacing them once and for all by functionally equivalent sequences of instructions in its own language, or (e.g., as an *Interpreter*) translating each instruction as it encounters it each time the program is run. But the common aim of all these modes of Automatic Programming is that as much as possible of the work necessary to close the gap between a mathematically formulated problem and a machine-coded computer program shall be given to the machine.

The contributions are of two main types. A few are devoted to problems of principle: the general aims, problems and likely future development of Automatic programming. The remainder are devoted to accounts of actual programming systems and the experiences derived from their use.

One point of principle consistently made in the general contributions concerns the necessity for flexibility in Automatic Programming systems—the languages should be readily extensible, notably by the addition of vocabulary, but also by the addition of syntax. But the desirability of enabling the computer to detect faults in programs, to indicate them in the problem-language, and then to accept program amendments in the problem-language, is not really brought out in these contributions, although it was a cardinal consideration in the specification of the SHARE 709 project. There is also a disappointing lack of agreement on fundamental questions—whether the object of Automatic Programming is faster coding, easier coding, easier comprehension, shorter programs, or more rapid program-development, and whether any of these objects are usually achieved. The desirability of a universal autocode is of course mooted, but the only really convincing argument brought in its favour is its potential usefulness in teaching. One contributor offers some definitions of the prevailing jargon terms, which apparently met with a substantial measure of agreement at the conference. In fact, a good number of interesting issues are taken up, but in the end the four or five general papers do not really seem to add up to a very penetrating analysis of the subject.

Perhaps the most immediately impressive thing about the remaining papers is their evidence of how much had already been accomplished two years ago—for over 20 Automatic Programming systems are described. There is a preponderance of systems of British origin, and whilst this is probably due to the fact that the conference was held here rather than, say, in America, it does indicate the existence of a lively interest in Automatic Programming in this country. Not all the contributors give accounts of their experiences in the use of the systems they describe, unfortunately, but one or two give very good analytical accounts, which seem to support fairly strongly the worthwhileness of Automatic Programming. The great notational variety lamented by the advocates of a universal autocode is certainly evident; on the other hand it is interesting to notice the considerable community of principle among the score or so of systems.

Several contributors partition Automatic Programming applications into the Scientific and the Commercial, and one wonders where Industrial applications fall within this dichotomy. To equate them with Commercial applications, as one contributor expressly does, is highly misleading, but to equate them with Scientific applications demands a wider conception of a scientific autocode than is afforded by an algebraic language equipped with subroutines for *log*, *cos* and *square-root*. Truth to tell, the attention given to Industrial applications in this book is quite small, and to Commercial applications almost negligible, simply because Automatic Programming had not spread far beyond Scientific

applications at the time of the conference. Things have changed, of course, since 1959, and there is good reason to hope that the second volume of this series will contain accounts of some of the commercial autocodes now extant, and of the various Automatic Programming systems devised for industrial simulation and statistical analysis.

A determined effort is made here and there in this book to relate the business of Automatic Programming to the work of A. M. Turing on computable numbers, on the rather vague grounds that he " first enumerated the fundamental theorem upon which all Automatic Programming is based ". To pay respect to Turing's contribution to mathematical thought is one thing; to include his famous 1936 paper as if it were more than tangentially relevant to the business of devising an Automatic Programming system for a digital computer, is misleading and seems, alongside the otherwise admirable aims and substantial achievements of the book, a little pretentious.

RAYMOND CUNINGHAME-GREEN

Theory of Differential Equations. By A. R. FORSYTH. Six volumes bound as three. $15. 1960. (Dover Publications, Inc., New York)

Forsyth's monumental work has been out of print for over a decade and mathematicians must be greatly indebted to Dover Publications for re-publishing Forsyth's work in three combined volumes.

Forsyth completed his task in 1906. Since then there have been numerous important developments in the theory of differential equations which have done much to rehabilitate the subject as a respectable branch of analysis and topology. But Forsyth's volumes, will I think, always have a permanent value as an historical presentation of the development of the subject from the beginning. They are indeed a mine of information about early researches into the subject and they also contain a most valuable collection of illustrative examples.

Turning over the pages of this great work, and bearing in mind the modern theory as expounded, for example, by Courant and Hilbert in volume II of their " Methoden der Mathematischen Physik " or of Tricomi's work " Equazioni a derivate parziali " one gets a curious impression that Forsyth exhibits some of the strange practices of the sleepwalker. In all the detailed mathematical expositions which occupy these hundreds of pages, he almost never makes a mistake. He handles complex algebraical situations with consummate ease and he triumphantly produces solutions of differential equations, like a conjurer producing rabbits out of a hat. But for all that, one reader at least, is left with the uncomfortable feeling that neither Forsyth nor the mathematicians whose work he describes, really knew what they were doing. Of course, this difficulty is largely due to the enormous progress which has been made in the subject, and to the triumphant satisfaction with which the pioneers regarded the construction of any solution of a differential equation, no matter how special and limited it might be.

The reviewer makes this criticism with some diffidence, and only with the intention of warning prospective buyers and readers of Forsyth's volumes that they will find them most valuable and intelligible in the light of developments made during the last thirty years.

G. TEMPLE

B-Methodes de Calcul, II. Calcul Symbolique. Distributions et Pseudo-Fonctions. By JEAN LAVOINE. Pp. 110. 1959. 10 NF. (Centre National de la Recherche Scientifique, Paris)

This little volume, reproduced by the lithographic method, gives a brief introduction to Schwartz's theory of distributions and an account of the application of this theory to Laplace transformations. The use of Schwartz' theory results in a considerable generalisation of the Laplace transformation and leads to a number of interesting and important results.

The second half of this book consists of an extensive table of Laplace transformations, especially those obtained by distribution theory. It thus forms a valuable supplement to the tables of MacLachlan and Humbert, Erdelyi and Magnus.

G. TEMPLE

Introduction to Mathematical Physics. By W. BAND. Pp. 326. 54s. 6d. 1959. (D. Van Nostrand Co., Ltd.)

This textbook—one of the " University Physics Series "—is designed for use in (American) courses on theoretical physics for college senior and graduate students. It is remarkable how many subjects are treated in only 326 pages. Its scope may be seen from the chapter headings: I Introduction: The language of mathematical physics (mostly vector theory); II The continuum theory of matter; III The molecular theory of matter; IV The theory of fields; V The theory of relativity (mainly the special theory); VI Quantum theory.

The mathematics is treated in the usual physical way, and modern methods, such as the use of δ-functions, are introduced. The emphasis is on the physics, and the use of mathematics to solve physical problems, and although some use is made of the " functions of mathematical physics ", the properties of these are usually assumed known. There are many interesting and stimulating problems at the end of each section, and anyone who works through these should obtain a real understanding of the subject. Some of the diagrams, such as those illustrating quadrupole charge distributions, or of a sixteen-dimensional vector, should go a long way towards removing the fears which such concepts usually induce at a first meeting.

The contents of this book are roughly similar to those of the much bigger " Theoretical Physics " by G. Joos (with the omission of much of classical physics); it differs mainly in not being so self contained, although this is apparently intentional as references for collateral reading are given at the end of each chapter. However it would have been

very helpful, especially for a student working on his own, if individual references had been given for the many results used without proof. Also it would have been nice to have a more definite indication of how thoroughly each topic was being dealt with. For instance, the two figures 7.3 and 7.4 (the first headed " The proof of Stokes' theorem ..."), and the statements on pages 38, 39 beginning " To demonstrate this theorem ..." and " This theorem is proved by ...", each consisting of only 8 lines of descriptive text and a single symbol (S and $d\sigma$), are surely not intended as a complete demonstration or proof of Stokes' theorem and Gauss' divergence theorem. A minor irritation is the occasional use of the notations $\nabla \times \nabla \times \mathbf{v}$, $\mathbf{v} \times \nabla \times \mathbf{v}$ without brackets.

A. WEINMANN

Markov Chains with Stationary Transition Probabilities. By KAI LAI CHUNG. Pp. x + 278. 120 s. 1960. (Springer, Berlin)

Markov chains with a denumerable infinity of states are at least twenty-four years old (Kolmogorov, 1936), but this is the first really comprehensive textbook on the general theory. Part I (discrete parameter chains, 113 pages) should find a vast circle of readers; the classification of states, the main limit theorem, the ratio theorems, " taboo " probabilities, and Doeblin's central limit theorem are all treated in great detail, and practically all of this material is eminently usable by statisticians, only a fraction of it being available in other books. I wish the publishers could be persuaded to market a cheap edition of this part of the book; there would be a great demand for it. Those who are familiar with the classical parts of the subject and with the author's previous contributions to it will find much here that is new; thus the " taboo " theory is generalised to allow an arbitrary set of excluded states, and the discussion of the ratio theorems is completed by a counter-example due to Dyson and Chung showing that even within a recurrent class of period one the ratio $p_{ij}^{(n)}/p_{kl}^{(n)}$ need not have a limit as $n \to \infty$. A closing section discusses almost closed and sojourn sets, and gives an account of most of Blackwell's and some of Feller's results, without however attempting to tell an unfinished story.

Part II (165 pages) is concerned with continuous parameter chains; a precise theory of their behaviour presents notorious difficulties, and the existing periodical literature is incomplete and difficult to follow even where it is correct. The author has put all serious students of the subject permanently in his debt by his masterly treatment of these difficult problems. His treatment is selective, as it necessarily must be within the space available; in particular the associated semigroup theory is barely touched on, and the necessity for an intrinsic compactification of the state-space is frequently insisted upon, but is not here attempted. The techniques employed are essentially those introduced in 1953 by Doob in his book *Stochastic Processes*, and some familiarity with the ideas of separability and measurability for a stochastic process are essential if the reader is to follow Chung's recreation in concrete mathematical terms of the highly original survey of the possibilities of Markovian behaviour sketched out by Paul Lévy in 1951.

The "continuous parameter" (often identified with time) ranges through the non-negative half-line $[0, \infty)$, thought of as a topological additive semigroup carrying an invariant (Lebesgue) measure. Thus it comes about that the pre-requisites for a thorough understanding of this subject include the group-theoretical properties of Lebesgue measure to be found in Titchmarsh's well known book on *The Theory of Functions* but excluded from most current texts on "measure theory". This fact is brought out very well in Chapters 1–3 of Part II, which contain much elegant analysis. Pride of place must be given to D. Ornstein's previously unpublished proof of Theorem II, 1–5: *If* (p_{ij}) *is a measurable transition matrix, then each* $p_{ij}(t)$ *is either identically zero or never zero for* $t>0$. The measure theory proper begins with Chapter 4, and includes extremely valuable extensions and elucidations of Doob's theorems on separability and measurability (especially in connexion with the important matter of Borel measurability for a process). What all this leads up to is the *Strong Markov Property*, an essential preliminary to a proper treatment of first-entrance formulae and post-exit processes. The first-entrance formulae then lead to significant analytical results (for example, the existence of a limit—finite and positive—for the ratio $P_{ii}(t)/P_{jj}(t)$ as $t\to\infty$, when i and j are intercommunicating states;* no analytical proof of this is yet known). Some further interesting analytical results are contained in the following section on "discrete skeletons", and here the author records the challenging observation that we still know of no necessary and sufficient condition for a discrete parameter chain to be the discrete skeleton of a continuous parameter chain. An answer to this problem would be of far-reaching significance in practical statistics.

Towards the end of Part II the argument swerves gradually towards questions concerning the existence, uniqueness, and synthesis of processes with a given set of initial time-derivatives, and semigroup theory lurks in the background, but hardly plays even a walking-on part. The author then discusses from the standpoint of a specification of sample functions the zoological garden of enormities previously studied by Kolmogorov and others in analytical terms. As a final shock, the Addenda contains Ornstein's 1960 theorem on the differentiability of transition probabilities.

The exhausted reader may find he has just enough strength left to write an appreciative letter to the author, who certainly deserves a handsome fan mail for this work of scholarship and illuminating comment.

D. G. KENDALL

The Science of Mechanics. By E. MACH. Pp. 634. 40s. 1960. (Open Court, La Salle, Illinois)

Mach's most characteristic and important ideas about the exact sciences are brought out very well in his study of mechanics. He

* Here $P_{ii}(t)=\int_0^t p_{ii}(\tau)\,d\tau$.

wished to construct a science with as few and as simple postulates as possible; he hoped to eliminate all non-instantial, or " metaphysical " concepts as he called them; and he wanted all remaining concepts to be explicable in terms of experience. A great many of the features of modern philosophy of science can be traced back to these ideas.

They are very clearly exemplified in Mach's discussion (Part II, Sections III and IV) of Newton's dynamics. There he shows that the equality of Action and Reaction is the fundamental law of Newtonian mechanics, since it describes the characteristic mechanical interaction between any chosen bodies. Furthermore it enables us to eliminate the concept of force as an undefined elementary concept in favour of acceleration; and to give a definition of mass that is neither circular like " quantity of matter ", nor question begging like equating the ratio of two masses to the ratio of the forces required to give them equal accelerations. Acceleration is something we experience and can measure. Force is not since it is known to us through acceleration. Using the principle of Action and Reaction Mach defines mass in the following way: " All those bodies are of equal mass, which, mutually acting on each other produce in each other equal and opposite accelerations ". This, he says, is simply a way of naming " an actual relation of things ".

In the final part Mach discusses, among many other philosophical topics, the assumption, so central to Nineteenth Century physics, that mechanical interaction is the fundamental physical interaction in terms of which all other kinds are to be explained. He argues that other kinds of interaction are evident enough, and that it is a gratuitous assumption to suppose that mechanical interactions are the " deepest ".

It is very pleasing, now that positivism is under heavy attack from many quarters, that so fine an edition of such an important book as this has been produced.

R. HARRÉ

Proceedings of Symposia in Applied Mathematics, Vol. IX: Orbit Theory. Pp. 195. $7.20. 1959. (American Mathematical Society, Providence, Rhode Island)

This book is the record of the papers presented at the Symposium on Orbit Theory held at New York University in April, 1957. As the editors remark, orbital theory, though one of the oldest branches of dynamics, is " neither moribund nor consummate ": recently the advances in high-speed computers and the urgent need to develop accurate orbital theories for earth satellites have stimulated many new contributions to the subject.

The longest paper in the book, a 70-page discourse by J. W. Siry on the choice of trajectory for satellite launching vehicles, is probably the most thorough analysis of the subject yet published, covering the theory of optimization and describing the climb paths chosen for the Vanguard satellites. In other papers P. Herget tersely presents the theory subsequently used in determining the orbit of Vanguard 1, F. L. Whipple surveys the problems of satellite orbits and tracking in a more general

way, and K. A. Ehricke contributes an excellent analysis of possible orbits in the Earth-Moon region. W. J. Eckert and D. Brouwer discuss new developments of classical planetary theory, and C. Graef-Fernández describes " Orbits in Birkhoff's Central Field ". Three papers deal with orbits in magnetic fields. The first is by E. D. Courant, on orbit stability in particle accelerators. The second, by S. Olbert, discusses the motion of cosmic-ray particles in the galactic magnetic fields. The third, by W. H. Bennett, records the results of an experimental simulation of Störmer orbits in the aurora, using a vacuum-tube technique, and it is interesting that one of the photographs shows an incoming stream of particles creating a strong " zone of radiation " round the earth, very much like that discovered a year later by Van Allen.

The individual papers are generally of high quality and the book constitutes a valuable survey of the subject. A collection of papers can never achieve the same balance as a textbook: this volume is no exception—chiefly because the lengths of the individual papers vary widely, and the longest one cannot really be classed as " orbit theory " at all—but it does approximate more nearly to a textbook than most such collections, and can be thoroughly recommended to anyone who is interested in the subject.

D. G. King-Hele

Boolean Algebras. By Roman Sikorski. Pp. ix, 176. DM 39.60. 1960. Ergebnisse der Mathematik und ihrer Grenzgebiete (Neue Folge) Heft 25. (Springer-Verlag, Berlin)

Since George Boole created in the middle of the 19th century an algebra to express the " laws of thought ", the study of Boolean Algebras has been intimately bound up with that of mathematical logic. In the middle of the 1930s Marshall Stone greatly extended the scope of the subject by showing its applications in topology and its connections with the algebraic theory of rings. Since then the interest and importance of Boolean algebras has been increasingly realized, and they have been studied by a growing army of investigators, both in their own right and for their applications, especially also to measure theory and functional analysis. Just how fast the subject has been growing can be judged from the bibliography of the book under review; more than two thirds of the 400 entries date from after the Second World War.

The appearance of this book in the well-known series " Ergebnisse der Mathematik " is, therefore, timely. The author's point of view is topological rather than algebraic (though algebraic topics are by no means neglected); the justification for this is to be found in the second chapter, which deals with infinitary operations and is the most weighty and original part of the book and occupies half of its bulk. The contents of the short first chapter, finite joins and meets, is also largely covered by the well-known books on lattice theory. An appendix describes briefly, without proofs but with copious references, the numerous applications of the theory to other mathematical disciplines. The extensive bibliography has been mentioned; there is also a list of

symbols, an author index, and a subject index. A number of misprints and small linguistic oddities will not seriously detract from the usefulness of the book.

B. H. NEUMANN

Electromagnetic Wave Propagation. Edited by M. DESIRANT and J. L. MICHIELS. Pp. 730 $22.00. 1960. (Academic Press, New York)

This volume consists of the papers read at one of the International Conferences sponsored by the Postal and Telecommunications Group at the Brussels Exhibition of 1958. As would be expected from its origin, the material is mostly of interest only to specialists in the field of radio-communication. About one sixth of the volume is of more fundamental physical or mathematical interest, dealing with the physics of the ionosphere or the mathematical theory of wave propagation, but even here the appeal is rather specialised. Topics discussed include ionisation by auroral particles, correlation of ionosphere data with solar noise, measurement of ionospheric turbulence, propagation over a flat earth, propagation in a stochastic medium, coherence and shock excitation of a straight wire.

E. WILD

Probability: An Introduction. By S. GOLDBERG. Pp. xiv, 322. $7.95. 1960. (Prentice-Hall, Inc.)

There has been a need for a book on probability combining the rigour and elegance of Feller's *Probability Theory* with a rather lower level of necessary mathematical knowledge. This book supplies that need, and, after reading it, it comes as no surprise that the author is a former student of Feller. By restricting himself to finite sample spaces, Professor Goldberg avoids the need for calculus and asserts that his book is suitable for "readers with only a good background in high school algebra and a little ability in the reading and manipulation of mathematical symbols."

The book opens with a chapter on elementary set theory with a natural emphasis on notation and manipulation. This is followed by a long chapter in which probability is introduced as a measure defined on a set, subject to certain intuitively obvious conditions, and concepts such as conditional probability and independence are also introduced. Chapter 3, entitled "Sophisticated Counting", is a short diversion into permutations and combinations. Chapter 4 introduces random variables and probability distributions, together with mean, variance, covariance and correlation. The final chapter discusses in detail the binomial distribution, and some applications with an industrial and business bias.

The more sophisticated sixth-former, who has any time left over from the compulsory examination topics, should be capable of reading and understanding this book, and it provides an admirable elementary introduction to the language and techniques of the more advanced texts on probability and statistics. Like many American books the quality of the paper and printing is high, as is the price.

F. DOWNTON

Statistics: An Introduction. By D. A. S. Fraser. Pp. ix, 398. 54s. 1958. (John Wiley and Sons: Chapman and Hall)

This introduction to mathematical statistics invites comparison with Mood's *Introduction to the Theory of Statistics* published in 1950, not only because the mathematical pre-requisites (calculus and matrix theory) are the same but because the ground covered is almost identical. Such differences as there are represent a decade's changes of emphasis in statistical theory and the fact that teachers expect more and more mathematical sophistication from their students as time passes. The reviewer preferred the present volume, but has to admit that two students on whom he tried this book were old-fashioned enough to prefer Mood; the results of a non-random sample of three can, however, hardly be considered significant.

The contents include probability and distributions (discrete and continuous), expected values including generating functions, properties of samples including the central limit theorem, normal distribution theory including the χ^2 and F distributions, estimation, testing of hypotheses, confidence intervals, regression analysis, experimental design (including factorial experiments), sequential analysis and non-parametric methods. There is also an appendix containing tables of the normal density function and its integral, and tables of the t, F and χ^2 distributions.

F. Downton

Introduction to Mathematical Statistics. By R. V. Hogg and A. T. Craig. Pp. ix, 245. 47s. 1959. (The Macmillan Company, New York)

Most introductory books in statistics start life as lecture notes for undergraduates. This one is no exception, being based on courses in the University of Iowa for advanced undergraduate students in mathematics and for certain first-year graduate students. The reader therefore needs a relatively good mathematical background; "a course in elementary analysis or in modern algebra seems to provide equally satisfactorily the desired mathematical maturity." Some intuitive knowledge of probability would also be a help to the reader, for the book opens with a chapter, only twenty-six pages long, in which probability, random variables, distributions and expected values (including generating functions) are covered.

After this somewhat abrupt beginning, the book develops on slightly unorthodox lines. A chapter on the binomial, Poisson, normal and χ^2 distributions, in which the idea of a parameter is introduced, is followed by a chapter on statistical (here called stochastic) independence, the manipulations associated with the sums of (independent) random variables and the introduction of the idea of a confidence interval. This chapter is rather misleadingly called "Introduction to Sampling Theory". The theory of point estimation, with emphasis on unbiased and sufficient statistics having minimum variance, is preceded by an excellent chapter on the transformation of (random) variables, where the F and t distributions are derived. It is followed by the establishment of the fundamental property of the independence of the mean

and variance in a sample from a normally distributed population and the resulting confidence intervals for the mean and variance of such a population. The remaining chapters deal with limiting distributions (including the Central-limit theorem), order statistics, test of statistical hypotheses (the Neyman-Pearson theory), an introduction to the theory of the analysis of variance, and finally the theory of the test for zero correlation in a bivariate normal distribution and of the χ^2 test for goodness of fit.

While this book has much to commend it as the basis for a course of lectures, not everyone will agree with the order in which some of the topics are discussed. It also raises the issue whether mathematical statistics is a branch of pure or applied mathematics. The approach here is uncompromisingly pure and all background material concerning the physical problems, for which the mathematics is a model, has been rigorously excluded. One must doubt whether a reader who depended solely on this book would, for example, really understand what constituted a random sample or what was the purpose of a significance test. It is, however, refreshing to read another introduction to the subject, which is not indistinguishable from all the others.

F. DOWNTON

The Theory of Storage. By P. A. P. MORAN. Pp. 111. 1959. 13s. 6d. (Methuen and Co., Ltd.)

The first of a new series of Monographs on Applied Probability and Statistics, Professor Moran's book deals with that part of congestion theory applicable to the study of reservoirs and similar storage systems. As the author points out, although his models " are much simpler than those which occur in practice " they do provide background knowledge for those engaged in practical problems.

After a general introduction and a chapter on the problem of inventories as stochastic processes, the main theme of the book—the theory of dams in discrete and continuous time—is discussed. These two chapters are followed by a chapter on the solution of these problems by simulation methods and a brief discussion of the programming of storage systems.

For such a short book the author has succeeded in including a surprisingly large amount of material and achieved his aim of providing background knowledge. In one respect, however, the author seems to be open to criticism. To include an attack, however justifiable, on the " extreme value " techniques associated in this text with a named author, without including any reference to the work of this author (E. J. Gumbel) in the bibliography is to do him less than justice. Readers interested in the other side of the story should refer to Gumbel's *Statistics of Extremes*, New York: Columbia University Press (1958).

F. DOWNTON

Statistical Estimates and Transformed Beta-variables. By G. BLOM. Pp. 176. 40s. 1958. (Almqvist and Wiksell: John Wiley and Sons: Chapman and Hall)

This specialised volume describes the author's researches into certain properties of random variables, which under suitable transformation have a distribution in the form of an incomplete beta function; that is, he is concerned essentially with the properties of order statistics. It is not a book for the statistical tyro, but for the mathematically inclined research worker with some preliminary knowledge of the sort of statistical problems in which order statistics are an appropriate tool.

The book is divided into three parts. The first part (two chapters) lays some foundations in the form of discussion and generalisation of the theory of estimation originally due to Fisher. Part II defines transformed beta-variables and discusses their general properties, including the properties of linear combinations of such variables. In Part III the results of Part II are used to obtain estimates of location and scale parameters using linear combinations of order statistics. The properties of such estimates are covered in considerable detail.

Mathematical statisticians will find this a valuable and stimulating book on this topic, although not one to be read casually.

F. DOWNTON

Probability and Statistics: the Harald Cramér Volume. Ed. by U. GRENANDER. Pp. 434. 100s. 1959. (Almqvist and Wiksell: John Wiley and Sons)

This volume contains nineteen studies in probability and statistics presented to Harald Cramér in honour of his 65th birthday. They are arranged in alphabetical order of authors but a subject order is more convenient here.

Eight contributions are reviews, the longest an 80 page essay by Wold on the rationale of constructing stochastic models in econometrics. He traces the path from deterministic models to causal chains and interdependent systems, which are then considered in detail as applied to the relationship between demand, supply and price, and compared on real data. Others also write on aspects of stochastic processes: Rosenblatt and Tukey on the estimation of spectra, from the viewpoints of the mathematician and communication engineer respectively; Grenander on non-linear operations on a process and differential and difference equations with random coefficients; Segerdahl on the evolutionary process which occurs in the collective theory of risk; and the threads are drawn together by Bartlett in his account of the impact of stochastic process theory on statistics. In addition, Wilks surveys non-parametric statistical inference; and Elfving the conditions for optimum experimental designs in linear regression.

A similar preoccupation with stochastic processes appears in the individual research work. The theory of Markov processes is increased by Doob, who writes about lower semimartingales; D. G. Kendall, who shows that the sequence of n-step transition probabilities ($n=0, 1, 2, \ldots$) can be given a Fourier-Stieltjes representation; and Lévy, who

proves that a process with only instanteous states can be constructed by means of Brownian movement. Masani and Wiener discuss the non-linear predictor for a stationary stochastic process, defined as a conditional expectation; and Masani examines when the absolutely continuous and jump-singular parts of the spectrum of a multivariate process correspond to the moving-average and deterministic parts. Using elementary arguments, Feller obtains the distribution of the position of the maximal term in sequences of partial sums of random variables; and Kac studies the probability that a stable process lies between fixed limits. Leaving probability for statistics, Fix, Hodges and Lehmann describe a restricted chi-squared test having greater power at specified alternatives than the ordinary test; Neyman constructs an optimum test within a class of asymptotic tests of a composite statistical hypothesis; Robbins studies sequential methods of estimating the mean of a normal population when the loss function is specified; and Anderson discusses models and estimation procedures for problems in the social sciences.

That the contributions should vary greatly in length, style, clarity and mathematical level was unavoidable, but the editor might perhaps have striven for a more uniform presentation: one contributor gives an extensive bibliography in which the volume number precedes the journal, while another, who remarks that the publications relevant to his review are widely scattered, gives only one reference, although he adopts a five-digit system of numbering the sections. The book has been printed in Sweden and the proportion of misprints is slightly more than usual in a Wiley publication. Apart from such minor defects, the high average quality of its reviews and researches will commend this book to all probabilists, and the volume is a suitable tribute to its distinguished recipient.

R. L. PLACKETT

Hydrodynamics. By D. H. WILSON. Pp. 149. 30s. 1959. (Edward Arnold)

Much ingenuity has been shown by the author in providing an acceptable introduction to so wide a field of hydrodynamics in just less than 150 pages. He has naturally had to be concise, occasionally perhaps curt, but the presentation is interesting throughout and the book, which is at the level of a first University course, will encourage students to go on.

What is required of vector analysis is stated without proof, and tensor analysis is not used. After some general theory, there are three chapters on the two-dimensional motion of inviscid fluids; these contain the formula of Blasius and its extension to unsteady flow, the Circle Theorem, rectilinear vortex motion with the Karman vortex street, and conformal transformations including Schwarz-Christoffel with applications to discontinuous motion and jets. In the chapters on axi-symmetrical motion and viscous flow it is not, of course, possible to give such a wide survey; the former includes the sphere theorems of Weiss and Butler, which compare with the two-dimensional Circle

Theorem, and the latter Stokes's and Oseen's solutions of the steady flow past a sphere, and a boundary layer solution of the flow past a flat plate with an assumed form for the velocity distribution in the layer. There is an adequate supply of examples for solution; at intervals throughout the book, points of mathematical detail are left for the reader to fill in, and this is a useful supplement to the formal examples.

W. R. Dean

Introduction to Matrices and Linear Transformations. By D. T. Finkbeiner, II. Pp. 248. 38s. 1960. (W. H. Freeman and Co. Ltd., San Francisco and London)

This introductory account of linear algebra stresses the conceptual side of the subject and is entirely modern in outlook. The main object of study is the theory of vector spaces and linear transformations; matrices appear as representations of linear transformations, and determinants play an altogether subsidiary role. The first six chapters cover the usual elementary topics, while Chapter 7 is concerned with characteristic roots, characteristic vectors, and diagonalization of matrices. The climax of the book is reached in Chapter 8, where the derivation of the classical canonical form is discussed in considerable detail. Having forged this powerful weapon, the author allows it to rust; for the only subsequent use made of the classical canonical form is in contexts where much cruder techniques would have done just as well or better. In the next chapter, entitled " Metric concepts ", the study of vector spaces is narrowed to that of euclidean spaces, and such notions as inner product, orthogonality, and quadratic form are developed. The treatment is excellent as far as it goes (except that the discussion of signature is not altogether easy to follow), but in the short space he allows himself, the author obviously cannot do justice to all the relevant topics. The final chapter deals with functions of matrices and systems of differential equations. There are two appendices; the first is concerned with general algebraic concepts (such as that of homomorphisms) while the second discusses A. W. Tucker's notion of combinatorial equivalence of matrices.

The selection of material in a field as rich as that of linear algebra obviously presents many thorny problems and must be governed, to some extent, by personal preference. Even so, the author's choice is at times puzzling. Certain topics, such as elementary operations on matrices, are treated at considerable length. Other topics (for example, orthogonal matrices, positive definite quadratic forms, hermitian forms) are discussed very summarily; and yet others (Laplace's expansion, normal matrices, polar factorization, unitary equivalence) are altogether absent. Though these omissions must be recorded, they should not be taken too seriously; for the right approach is more important than encyclopaedic completeness. Thus, in spite of minor reservations, the book may be recommended as a lucid and competent account of linear algebra.

L. Mirsky

Tables of the Riemann Zeta Function. By C. B. Haselgrove in collaboration with J. C. P. Miller. Pp. xxiii, 80. 50s. 1960. Royal Society Mathematical Tables, Volume 6. (Published for the Royal Society at the University Press, Cambridge)

It was in 1859 that Riemann enunciated his famous hypothesis, that the non-trivial zeros of $\zeta(\sigma+it)$ all lie on the line $\sigma=\frac{1}{2}$. A century later the present tables had already for some time been set up in type. They do not include sufficiently high values of t to extend outside the region in which Riemann's hypothesis has so far been proved to be true (as Lehmer has shown it to be for the first 25000 zeros, lying in $0<t<21943{\cdot}645$), but they have already sufficed to disprove two more modern conjectures.

Table I (pages 2–21) lists to 6 decimals for $t=0(\cdot 1)100$ the real and imaginary parts of $\zeta(\frac{1}{2}+it)$ and $\zeta(1+it)$, as well as, for the former function, a signed modulus $Z(t)$ and corresponding phase-indicator $\theta(t)$ such that $\zeta(\frac{1}{2}+it)=Z(t)e^{-i\pi\theta(t)}$ and $Z(t)$ changes sign at its zeros.

Table II (pages 22–57) lists $Z(t)$ to 6 decimals for $t=100(\cdot 1)1000$. Values of the real and imaginary parts of $\zeta(\frac{1}{2}+it)$ are not printed, but these, along with values of $Z(t)$ for $t=0(\cdot 1)2100$, have been placed in the Royal Society Depository for Unpublished Mathematical Tables, No. 65.

Table III (pages 58–70) gives to 6 decimals the first 1600 zeros γ_n of $Z(t)$, lying in $0<t<2090{\cdot}4$, and to 5 decimals the corresponding values of $|\zeta'(\frac{1}{2}+i\gamma_n)|$. For $n=1(1)650$, ϕ_n and g_{n-1} are also tabulated, to 6 decimals; here $\phi_n=\pi^{-1}\,\mathrm{ph}\,\zeta'(\frac{1}{2}+i\gamma_n)$, where ph denotes phase (argument), and g_n is the value of t defined by $\mathrm{ph}\,\{\pi^{-\frac{1}{2}it}\Gamma(\frac{1}{4}+\frac{1}{2}it)\}=n\pi$, which makes $\zeta(\frac{1}{2}+ig_n)$ real.

Table IV (pages 71–75) gives $Z(t)$ to 6 decimals in the four special ranges 7000($\cdot$1)7025, 17120($\cdot$1)17145, 100000($\cdot$1)100025 and 250000($\cdot$1)250025. Within these ranges, all zeros γ_n are also listed to 6 decimals, together with corresponding values of $|Z'(\gamma_n)|$ to 5 decimals. The first two ranges were chosen on account of Lehmer's discovery of pairs of close zeros near 7005 and 17144, and also to exhibit a high maximum of $Z(t)$ near 17123; the last two were chosen almost at random as examples of high t.

Table V (pages 76–80) gives $\pi^{-1}\,\mathrm{ph}\,\Gamma(\frac{1}{4}+it)$ to 6 decimals for

$$t=0(\cdot 1)50(1)600(2)1000.$$

A short introduction gives much information about formulae and methods of computation. It would have been quite impracticable to compute the tables on desk machines, of which very little use was made; the computation was performed mainly on *EDSAC* at Cambridge and the Mark I computer at Manchester University. Only recent work (culminating in 1956) of Titchmarsh and then Lehmer on the Riemann–Siegel formula allowed an adequate check to be made; tables of coefficients are given.

The volume under review contains the results of one of the most striking applications so far made of electronic computers in the field of pure mathematics.

A. Fletcher

Analytical Elements of Mechanics, Vol. I. By THOMAS R. KANE (Pp. ix, 250. 1959. \$4.75. Academic Press)

The four chapters of this book are successively Vector Algebra, Centroids and Mass Centres, Moments and Couples, and Static Equilibrium. The contents list is decomposed clearly into paragraphs and, besides an index, there are sets of problems at the end, with answers. Also many illustrative problems are solved in the text which is liberally supplied with good illustrations.

The mechanics is two and three-dimensional, and is treated vectorially throughout. From place to place there are detailed notation lists stating specifically the symbols to be used in the sections following and this, together with some of the notation, makes the book appear visually more complex than it is. The notation, e.g. $\mathbf{M}^{\mathbf{V}/A}$, the moment of $\mathbf{V}$ about A, is very tightly bound to the text and seems to creak until one becomes familiar with it.

The title and chapter headings state clearly what one expects to find in this book, but the presentation and examples might surprise the reader who has not studied the preface. Here, the author states ... " My objectives in writing it were to provide students and teachers wih a text consistent in content and format with my ideas regarding the subject matter and teaching of mechanics, and to disseminate these, ideas." He goes on to state his belief that a first aim of a mechanics course is to prepare students for later studies in other subjects, and further that the ultimate purpose is to teach the student to solve physically meaningful problems in a variety of fields. These two aims, more particularly the second, colour the book throughout including the problem sets which are of a much more mechanical nature than is usually found in books on Statics. The idea of centre of gravity is not used in Chapter 2 whilst, in Chapter 3, the word " force " is used occasionally in illustrative problems but the text deals with the properties of moments of bound vectors about points and lines. Indeed equilibrium, *per se*, is not mentioned until the last chapter, the author reasoning in his preface that intuitive definitions of force are not good for the pupil. Thus force is presented in an axiomatic fashion as a bound vector, and space, mass and time are taken for granted. I do not agree that this is the best approach, particularly at the level of the text; but the point, as a teaching one, is debatable. After dealing with mutual gravitational attractions of particles and continuous bodies in order to convey the idea of the weight of a body acting at its centre of gravity, the rest of the chapter deals with the equilibrium of various mechanical devices, including problems on chains and friction.

Certainly, this book is not out of the traditional mould, but I do not think that it is any improvement on existing texts covering the same ground. As the author states, the book is certainly not one to be used by the student alone, but for the use with the teacher. More than that, however, I do not think it will become a popular book with every teacher.

R. BUCKLEY

Tensorrechnung in analytischer Darstellung. I. Tensoralgebra. By A. DUSCHEK and A. HOCHRAINER. Fourth Edition. Pp. 171. $5.70. 1960. (Springer-Verlag, Wien)

This is a first-class outline of the general theory up to and including second order tensors and the ϵ-tensor. The exposition is clear and there is an attractive elementary chapter on orthogonal transformations and the rigid displacement group.

R. BUCKLEY

Variationsrechnung. By M. Miller. Pp. 133. DM 8.10. (B. G. Teubner-Leipzig)

An introductory book which requires no more than an elementary knowledge of differential and integral calculus, this is based on the idea that in the Calculus of Variations, an ounce of practice is worth a pound of theory. The problem is stated, and the Euler equations derived, and the theory applied to a series of problems in geometry and mechanics. The last chapter discusses approximate solutions.

P. HOLGATE

The Diffusion of Counting Practices. By A. Seidenberg. Pp. 86. $2.50. 1960. (University of California Press. Cambridge University Press)

This scholarly paper aims to show that 2-counting, that is counting 1, 2, 2 + 1, 2 + 2, 2 + 2 + 1, 2 + 2 + 2, ... is older than 5-, 10-, or 20-counting. The important point, often overlooked, that counting is not merely keeping a tally (which corresponds to counting by 1's) but is a process of organisation, (or to use the reviewer's own expression) a transformation of signs, is firmly established.

R. L. GOODSTEIN

The Philosophy of Mathematics. By S. KÖRNER. Pp. 198. 12s. 6d. 1960. (Hutchinson)

This is quite a remarkable book, remarkable both for its clarity of exposition and for its insight; it is a book of thoughts about mathematics, and the relation of mathematics to the real world. Starting with an account of some of the older views of the nature of mathematics, views held by Plato, Aristotle, Leibniz and Kant, the author proceeds to describe and discuss Russell and Frege's logicism, Hilbert's formalism and Brouwer's intuitionism. Without entering into extensive technical detail the essential nature of these systems is clearly conveyed and each is judged, and in my view rightly judged, to fail to distinguish satisfactorily between mathematical and empirical properties. The final chapter outlines a first draft of a logic of inexact concepts; whether one fully accepts the author's characterisation of mathematics as the science of purely *exact* concepts, or not, his separation of mathematical and empirical properties is undoubtedly correct

and the problem of the relationship of mathematics to its application in nature which this separation brings to light is one which, as Professor Körner insists, has so far failed to receive the attention it merits.

R. L. GOODSTEIN

Naive Set Theory. By P. R. HALMOS. Pp. 104. 26s. 6d. 1960. (D. Van Nostrand Co. Ltd.)

Axiomatic Set Theory. By P. SUPPES. Pp. 265. 45s. 1960. (D. Van Nostrand Co. Ltd.)

There is nothing naive about the first of these books; the adjective is used in the title only to stress the absence of formal-logic notation. Both books present versions of Zermelo-Fraenkel set-theory with axioms of extension, specification, pairing, union, power, infinity and choice. Halmos' account is rather more readable but is marred by the author's view that set theory is a trifle to be absorbed swiftly and forgotten. There is nothing of the exciting adventure in ideas which makes Fraenkel's *Set Theory* so valuable an introduction; perhaps only an author who participated in the discovery of a subject can convey this kind of excitement. Suppes' book is rather more extensive, contains numerous examples and makes a modest use of formal logic sufficient to rescue the axiom of specification from the vagueness which surrounds it in Halmos' account. Although they cover very much the same ground, relations and functions, cardinals, ordinals, transfinite induction and recursion, the books are written from such diverse standpoints that each serves to complement the other, and the pair form a good introductory text.

R. L. GOODSTEIN

La Consistance des Théories Formelles et le Fondement des Mathématiques. By M. MEIGNE. Pp. 115. 12 NF. 1959. (Blanchard, Paris)

After an informal account of Godel's incompleteness theorem for Arithmetic, and Gentzen's and Ackermann's proofs by transfinite induction that Arithmetic is free from contradiction the author explains his view that since axiomatisation has failed to provide a foundation for mathematics, a foundation must again be sought in mathematical intuition; he proposes a view of mathematics as a succession of structures, based on an intuitive theory, each structure forming the "intuitive model" of the one above. A fuller account of the author's system is given in his book "Récherches sur une logique de la pensée créatrice en mathématiques". Fundamentally, Meigne's view is that a formal system is a formalisation of "Something", and that ultimately this something must be an intuitive theory, which is rather like assuming that every painting must be a painting *of something*, whether in the real world, or the painter's mind.

R. L. GOODSTEIN

An Introduction to Linear Programming and the Theory of Games. By S. VAJDA. Pp. 76. 1960. (Methuen, London)

The solution of simple problems in linear programming by means of coordinate geometry is the main aim of this helpful little book, followed by a glimpse of Game Theory. Two appendices contain proofs of the Fundamental Duality Theorem and of the theorem that it is always possible to find a solution (if mixed strategies are admitted) to any game in which both players have a finite number of strategies.

R.L.G.

Introduction to Homological Algebra. By D. G. NORTHCOTT. Pp. 282. 42s. 6d. 1960. (Cambridge University Press)

The branch of algebra known as homological algebra is of very recent origin, although its methods have been used by topologists for the last two decades. Its results first achieved book-form with the publication of Cartan and Eilenberg's book "Homological Algebra" in 1956 and it was in this book that a systematic development of the subject first appeared. Cartan and Eilenberg's account of the subject is not an easy one to read, since it contains a great number of results packed into a relatively small number of pages. Professor Northcott's book has a more modest aim, that of developing the basic ideas of the subject in a leisurely manner and illustrating the use of the methods with a few applications. Most of, but not all, the material is already available in Cartan and Eilenberg.

The book contains 10 chapters, the first two being, respectively, introductory chapters on modules and on tensor products and groups of homomorphisms. The accounts of both these topics are clear but offer no surprises. Chapter 3 commences the study of Homological Algebra since it is concerned with the basic notions of a category and of a functor. Next the author considers the theory of homology in Chapter 4 and here an innovation appears since Northcott considers homology as a function of a three-termsequence (A) $A_2 \xrightarrow{\lambda} A_1 \xrightarrow{\mu} A_0$ with $\lambda\mu = 0$, $\mathrm{H}(A)$ being defined as the quotient group Ker λ/Im μ. This idea is due to Yoneda, and to the reviewer seems to offer considerable advantages. Chapter 5 contains an account of projective and injective modules, with a very clear proof of the theorem that any module can be imbedded in an injective module. Further, the chapter also deals with the basic properties of projective and injective resolutions. Chapter 6 gives the definition and development of derived functors and the related theory of connected series of functors which are the fundamental notions underlying homological algebra. In Chapter 7, the Torsion and Extension functors are introduced as examples of derived functors and they are applied to the theory of global dimension of rings. Chapter 8 is devoted to certain auxiliary results to be used in the last two chapters. These two chapters are concerned with applications. Chapter 9 is concerned with modules over Noether rings and its *pièce de resistance* is Serre's proof that regular local rings are the only local rings of finite global dimension. Chapter 10 is concerned with the homology and cohomology of groups.

This book is a very much more elementary account of Homological Algebra than has hitherto been available. As such, its appearance is greatly to be welcomed. Its value is further enhanced by the notes at the end of the book. The book can be strongly recommended to those algebraists, geometers and topologists who wish to understand what homological algebra is about.

D. Rees

Special Relativity. By W. Rindler. Pp. 186. 10s. 6d. 1960. (Oliver and Boyd, Edinburgh)

This is a clear and useful treatment that covers the traditional scope but with certain novelties. There is commendably more use of 4-dimensional methods than is common at this level; there is also modest use of tensors for linear transformations in 3- and 4-dimensions. There are instructive exercises on the work of each chapter, including some that are particularly thought-provoking.

W. H. McCrea

Axiomatics of Classical Statistical Mechanics. By Rudolf Kurth. Pp. 180. 45s. 1960. (Pergamon Press, Oxford)

The professed aim of this book is the construction of classical statistical mechanics as a deductive system, founded on the equations of motion and a few well-known postulates which formally describe the concept of probability. From the preface, one suspects that one is to be treated to a display of careful linguistics: " axiomatics " and " completeness " are defined in the first paragraph. However, phrases such as " appear obvious " and " is plausible " soon creep in, though Dr. Kurth often shews his uneasiness at their use by enclosing them in inverted commas.

It is claimed that only a knowledge of the elements of calculus and analytical geometry need be known by the reader, and the second chapter, which takes up a quarter of the book, describes the mathematical tools which are used throughout the rest of the work. Most readers will find this chapter either unnecessary or too difficult. Dr. Kurth has, in fact, fallen into the usual trap which awaits those who are bold enough to include mathematical introductions in their books.

The rest of the book, however, is good, though the number of misprints is rather large. Much of the work has not been previously published, and the whole is presented in a scholarly fashion. The forms in which some well-known theorems appear may come as a salutary shock to some physicists.

Chapter Three deals with Liouville's theorem and the Ergodic Hypothesis, and Chapter Four with the initial distribution of probability in phase space. I thought a more detailed consideration of the effects of the replacement of the probability set function by a point function might have been interesting. The Fifth and Sixth Chapters deal with

time-dependent and -independent probability distributions respectively. The last chapter relates the work to thermodynamics.

I recommend this book to those who have doubts about the foundations of statistical mechanics. Dr. Kurth will probably enable them to rid themselves of some of these; and no doubt he will suggest to them others of which they were not previously aware.

W. E. Parry

Milieux Conducteurs ou Polarisables en Mouvement. By H. Arzeliès. Pp. 347. 58 NF. 1959. (Gauthier-Villars, Paris)

Earlier volumes of these *Études relativistes* by Professor Arzeliès have been reviewed in *Gazette*, XLI, XLIII, XL. Each deals with some department of relativity theory or its application along similar lines—a discursive introduction, a few chapters giving a fairly formal development of the main topics with a rather copious commentary and closing with historical notes and bibliographies, appendices dealing with mathematical matters. Evidently, however, the author does not intend the sequence of books as a whole to be organized according to any particular plan; the present book does not provide even a list of the others.

Chapter 1 recalls the equations of electromagnetism valid in media of various sorts at rest in an inertial frame. The rest of Part I of the book deals with such results in cases where the medium is in uniform motion relative to the inertial frame of interest. Part II deals with electromagnetism in a gravitational field and with media in more general states of motion. This means that the electromagnetic theory is generalized so as to hold good self-consistently in a general Riemannian space-time. The main detailed application is to the field of permanent magnets in uniform rotation relative to an inertial frame (Chapters 8, 9). In this book, Professor Arzeliès uses tensor methods more freely than in earlier ones. It should prove a useful book of reference for the somewhat technical application of relativity theory with which it is concerned.

W. H. McCrea

BRIEF MENTION

Transcendental and Algebraic Numbers. By A. O. Gelfond. Pp. 190. $1.75. 1960. (Dover, New York; Constable, London)

Although two of Gelfond's papers on the approximation of algebraic numbers and on algebraic independence have been previously published in English by the American Mathematical Society this book contains in addition to the results of these papers other important work of the author's not hitherto available in English. The translation from the First Russian Edition was made by L. F. Boron.

A Treatise on the Calculus of Finite Differences. By G. Boole. Pp. 336. $1.85. 1960. (Dover, New York; Constable, London)

This reprint of the second edition appears exactly a hundred years after the first publication of a book that was a signpost for future development.

The Calculus of Finite Differences. By G. Boole. 4th ed. Pp. 336. \$1.39. 1960. (Chelsea, New York)

This edition differs from the second and third primarily in notational changes.

Textbook of Algebra. By G. Chrystal. 2 volumes, 1,235 pages. Each volume \$2.95. 1959. (Chelsea, New York)

This reprint of the sixth edition makes available again books which were widely used in this country at the turn of the century, and which contain one of the most complete accounts of continued fractions in English.

Entropy and the Unity of Knowledge. By P. T. Landsberg. Pp. 27. 3s. 6d. 1961. (University of Wales Press)

An Inaugural Lecture delivered at University College, Cardiff on the 29th November, 1960.

Recherches sur l'Analyse Indéterminée et l'Arithmétique de Diophante. By E. Lucas. Pp. 92. 8 NF. 1961. (Blanchard, Paris)

Amongst the equations considered are $x^3+y^3=Az^3$, for certain values of A; $ax^4+bx^3+cx^2+dx+e=y^2$; $3x^4-y^4=2z^2$; $2x^4-3y^4=-z^2$; $x^5+y^5=Ay^5$; and the simultaneous equations $v^2-6u^2=w^2$, $v^2+6u^2=w^2$. Amongst other results obtained it is proved that the sum of the first n numbers and the sum of the cubes of the first n odd numbers is never a cube, a fourth or a fifth power.

Algebraic Equations. By E. Dehn. Pp. 208. \$1.45. 1960.

Introduction to the Theory of Linear Differential Equations. By E. G. C. Poole. Pp. 202. \$1.65. 1960.

Theory of Maxima and Minima. By H. Hancock. Pp. 193. \$1.50. 1960.

Calculus of Variations. By A. R. Forsyth. Pp. 656. \$2.95. 1960.

The Theory of Equations. By W. S. Burnside and A. W. Panton. Vol. 1; Pp. 286. \$1.85. Vol. 2; Pp. 318. \$1.85. 1960.

Great Ideas of Modern Mathematics: Their Nature and Use. By J. Singh. Pp. 312. \$1.55. 1959. (Dover, New York)

Apart from the last volume which was published simultaneously with the British Commonwealth Edition reviewed by T. A. A. Broadbent in *Gazette* XLIV, p. 306, these volumes are all reprints. Dehn's *Algebraic Equations* is an introduction to Galois Theory, which despite certain out-of-date terms and expressions is still valuable for the many worked examples it contains. Poole's *Differential Equations*, a standard work for many years, was reviewed by E. L. Ince in *Gazette* XXI, 64. Hancock's criticisms (in 1917) of some older text books' fallacious treatment of maxima and minima apply with equal force to texts in current use. Forsyth's *Calculus of Variations* is in the author's characteristically monumental style. Burnside and Panton's *Theory of Equations* remains an outstanding work in its field.

Exponentially Distributed Random Numbers. By C. E. CLARK and B. W. HOLZ. Pp. 249. 52s. (John Hopkins Press and Oxford University Press)

This book presents a table of pseudo-random numbers from the exponential distribution whose probability density is e^{-x}, $0 \leqslant x < \infty$; the validity of the generation process is proved and there is a summary of the results of tests of randomness which were applied.

Ingenious Mathematical Problems and Methods. By L. A. GRAHAM. Pp. 237. 1960. $1.45. (Dover, New York. Constable, London)

This excellent collection contains 100 problems with solutions. The problems range from familiar ones like finding two integral sided right angled triangles with the same area, to problems like that of arranging 9 equal gear wheels to give a gear ratio of 6 : 1. Problem 53 is Morley's trisector theorem; problem 62 on the collinearity of centres of similitude is attributed to J. E. Sweet but is a good deal older; the counterfeit penny problem, number 61, is wrongly dated 1945, since as Littlewood says in his *Miscellany* the problem was well known here during the war.

Cours de Mathématiques. By J. BASS. Second Enlarged Edition. Vol. 1. Pp. 608. 58 NF. Vol. 2. Pp. 440. 46 NF. 1961. (Masson et Cie, Paris—6)

Amongst many changes in the second edition of this valuable textbook is a new section on Boolean Algebra, with a note on applications to circuit theory, an extension of the chapter on tensors to introduce the concepts of covariance and contravariance, and an expanded account of the elementary theory of harmonic functions of which there was just a sketch in the first edition.

The Methods of Plane Projective Geometry based on the use of General Homogeneous Coordinates. By E. A. MAXWELL. Pp. 230. 13s. 6d. 1961. Students Edition. (Cambridge University Press)

An unaltered reprint of the first edition.

Elementary Differential Equations. By W. T. MARTIN and E. REISSNER 2nd Ed. Pp. 331. 51s. 1961. (Addison-Wesley, Reading, Mass., U.S.A., and London, England)

The first edition of this useful book was reviewed in *Gazette*, XLI, p. 155. The new edition contains a section on differential operator methods, and more than twice as many exercises.

Lectures on the Calculus of Variations. By O. BOLZA. 2nd Ed. Pp. 271. $1.19. 1961. (Chelsea, N.Y.)

This second edition is a reprint of the first edition incorporating a number of additions, corrections and notational improvements. It is printed on a newly developed high grade paper.

Spherical Astronomy. By W. M. SMART. 4th Ed. Pp. 430. 22s. 6d. 1961. (Cambridge University Press)

In the 30 years since the first edition appeared this book has seen four reprints of the fourth edition.

Quantum Mechanics. By ENRICO FERMI. Pp. 171. 12s. 6d. 1961. (University of Chicago Press, London)

This volume is a photographic reproduction of hand-written lecture notes for a course of lectures which Fermi gave in 1954.

A Short Account of the History of Mathematics. By W. W. ROUSE BALL. Pp. 522. $2.00. 1961. (Dover, New York. Constable, London)

An unabridged and unaltered republication of the author's last revision which appeared in 1908.

Invariant. Edited by G. W. H. SMITH, Jesus College, Oxford. No. 1. Michaelmas 1961. Pp. 20. 1s.

This is the first number of a new Journal by the Oxford University Student Mathematical Society. It contains amongst other items the presidential address to the Society by Dr. I. W. Busbridge on *Oxford Mathematics and Mathematicians from John of Holywood* (d. 1244) *to Henry Whitehead* (1905–1960), a proof by P. M. Neumann that there are analytic functions, real on the real axis, rational at all rational points on the real axis, but not polynomials, and an article by J. N. Crossley on the foundations of mathematics (a reply to which is to be printed in the next issue).

A new Report on the teaching of arithmetic is being prepared by a sub-committee of the Mathematical Association's Teaching Committee. This Report will be concerned largely with the arithmetic taught in grammar schools. The sub-committee would like to get in touch with teachers who are doing interesting work which might well be of interest to other teachers and which is not usually included in a grammar school course. Such teachers are requested to write to the secretary of the Arithmetic Sub-committee, J. K. Backhouse, 49 The Moors, Kidlington, Oxford.

THE MATHEMATICAL ASSOCIATION

The fundamental aim of the Mathematical Association is to promote good methods of Mathematical teaching. Intending members of the Association are requested to communicate with one of the Secretaries. The subscription to the Association is 21s. per annum and is due on January 1st. Each member receives a copy of the *Mathematical Gazette* and a copy of each new Report as it is issued.

Change of address should be notified to the Membership Secretary, Mr. R. E. Green. If copies of the *Gazette* fail to reach a member for lack of such notification, duplicate copies can be supplied only at the published price. If change of address is the result of a change of appointment, the Membership Secretary will be glad to be informed.

Subscriptions should be paid to the Hon. Treasurer of the Mathematical Association.

The Library of the Mathematical Association is housed in the University Library, Leicester.

The address of the Association and of the Hon. Treasurer and Secretaries is Gordon House, 29 Gordon Square, London, W.C.1.

SOUTHAMPTON AND DISTRICT BRANCH

Report for the Session 1960–61

The membership of the branch continues to increase. The average attendance at meetings this year has been 60.

At the Annual General Meeting on 25th October, 1960, the following committee was elected—President: Professor E. T. Davies; Vice-President: Mr. T. A. Jones; Secretary and Treasurer: Dr. F. Rhodes; University Representative: Professor B. Thwaites; also Mr. B. D. Dagnall, Miss H. Bromby, Miss M. D. Hemingway and Mr. P. E. Bryant. After the business meeting Mr. B. D. Dagnall gave a lecture entitled " Oily Arithmetic ", in which he discussed the mathematical techniques used in the oil industry.

Professor E. T. Davies delivered his presidential address on November 24th, 1960. Under the title " Some Elementary Examples of Generalization in Mathematics " he investigated some ideas which permeate both elementary and advanced mathematics.

On January 20th, 1961, Professor H. Bondi spoke on " The Space Travellers' Youth ". He gave an excellent exposition of the ideas which underlie the theory of relativity.

Dr. N. Mullineux lectures on " Elementary Integral Equations " on March 10th, 1961.

At a joint meeting with Southampton University Mathematical Society on 15th May, 1961, Dr. T. J. Willmore lectured on " Axiomatics ". He discussed the development of the understanding of Euclidean and other geometries.

A large audience gathered on June 27th, 1961, to hear Professor W. W. Sawyer speak on " Some Recent Controversies in the Teaching of Mathematics in the United States of America and their Lessons for Britain ".

LEICESTER AND COUNTY BRANCH

Report for the Session 1960–61

Speakers at Branch meetings held during the past session were as follows:

- Mr. W. J. Langford, Headmaster of Battersea Grammar School, on " Arithmetica ".
- Mr. Cyril Hope, of the City of Worcester Training College, on " The Teaching of Modern Mathematics in Schools ".
- Dr. E. J. F. Primrose of Leicester University, our President, on " Dynamic Geometry ".
- Mr. Donald Gratton, a B.B.C. producer of Television Schools programmes, on " School Television Mathematics ", a talk illustrated by B.B.C. telerecordings.

Mr. F. C. Downton, of Leicester University on " Queues ".

Professor W. W. Sawyer, on a visit to this country from Wesleyan University, Middletown, Connecticut, U.S.A., on "Controversies about the teaching of Mathematics in U.S.A. and their significance for Britain ". We were particularly pleased to welcome Professor Sawyer back to Leicester as it was he who was the prime mover in the formation of our Branch in 1947.

In November we held our annual quiz between teams from the sixth forms of City and County Grammar Schools before an audience of about two hundred Branch members and sixth-formers.

At an interesting and well-attended meeting in January four teachers from Leicestershire schools opened a discussion on " The use of structural material in the teaching of Mathematics ". They described the apparatus they are using and the work they are doing in their schools on the lines suggested by Dr. Dienes.

An additional meeting was held in May at the request of Mr. A. P. Rollett of the Ministry of Education to discuss the possible lightening of Advanced level syllabuses. The general opinion was that, with a few minor exceptions, lightening would mean weakening and that this would be a most retrograde step at a time when there is so great a demand for more and better Mathematicians, Scientists and Technologists.

Branch officers for the 1961–62 session are as follows:

President: Miss F. E. Billsdon.
Vice-Presidents: Professor R. L. Goodstein, Mr. W. Flemming, Mr. J. W. Hesselgreaves, Dr. E. J. F. Primrose.
Secretary: Mr. W. E. Remington.
Treasurer: Mr. L. F. Clarke.

W. E. Remington, *Hon. Sec.*

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